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Experiments in Corruptibility

Iryna Momotenko

Dissertation

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Dissertation Committee:

Andreas Ortmann (UNSW, Chair)

Randall Filer (Hunter College)

Jan Zapal (CERGE-EI)

Referees:

Klaus Abbink (Monash University)

Björn Frank (University of Kassel)

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Abstract

This thesis theoretically, but mostly experimentally, investigates corruptibility and ways to fight corruption.

In the first chapter, I investigate if an individual is more likely to behave corruptly if she believes that the majority of his/her peers are behaving in a similar manner. I also study what motivates an individual to follow the behavior of others more: knowing that the majority behaves corruptly or the willingness to fulfill the expectations of one's peers. I use a one-shot reverse public goods experiment to provide answers to these questions. The results suggest the existence of a peer effect of corrupt behavior. Expectations of the number of corrupt peers are found to be the main source of subjects' anti-social behavior.

In the second chapter, I question the rationale behind previous research results on the relationship between officials' wages and the level of corruption. In the light of counterintuitive empirical findings by Schulze et al. (2016) and Chen and Liu (2018), I investigate the wages-corruption relationship by means of a laboratory experiment, using a robust version of a bribery game with four different relative wages for public officials. I also introduce a new method of assigning different wages to participants, which helps to reduce self-selection based on ability, but creates a sense of entitlement to the endowment. The main findings are that (i) the effect of wages on officials' propensity to accept bribes is negative, and (ii) the effect of wages on officials' propensity to act on bribes (i.e., reciprocate a bribe by providing a corrupt service) is U-shaped.

In the third chapter, I propose an explanation of the positive, and U-shaped, wage-corruption relationship found in previous research: When an official's salary rises beyond a certain threshold, their self-perceived socio-economic status (SSES) increases and they become less averse to corrupt behavior. I investigate this theory using a laboratory experiment in which public officials can accept a bribe and then decide whether to act on it. The act benefits the briber but imposes externalities on social welfare. I manipulate officials' SSES experimentally to test if there is some such effect and when it kicks in. The results reported in this paper suggest that the effect of SSES on the propensity of officials with the highest wage to accept and reciprocate bribes is positive and significant if a bribe is high enough.

Abstrakt

Tato práce teoreticky, ale většinou experimentálně, zkoumá koruptibilitu a způsoby boje proti korupci.

V první kapitole se zabývám otázkou, zda se jednotlivec bude chovat více korupčně, pokud se domnívá, že se většina jeho vrstevníků chová korupčně. Také studuji, co více motivuje jednotlivce, aby následoval chování druhých: vědomí toho, že se většina chová korupčně, či vůle splnit očekávání svých vrstevníků. Pro odpovědi na tyto otázky používám jednorázový reverzní verzi experimentální hry o veřejné blaho (one-shot reverse public goods experiment). Výsledky naznačují existenci vzájemného (peer) effektu korupčního chování. Počet zkorumpovaných vrstevníků je hlavním zdrojem protispolečenského chování jedinců.

Ve druhé kapitole se zabývám opodstatněním závěrů předchozího výzkumu ohledně vztahu mezi mzdami úředníků a úrovní korupce. Ve světle neintuitivních empirických závěrů Schulze a kol. (2016) a Chena a Liu (2018) zkoumám vztah mezi mzdami a korupcí prostřednictvím laboratorního experimentu, který využívá robustní verzi hry úplatkářství se čtyřmi různými mzdami pro úředníky. Zavádím také novou metodu přiřazování platu účastníkům experimentu, která pomáhá snížit sebe-výběr na základě schopností, ale vyrůstá z pocitu nároku na peníze. Hlavní zjištění jsou, že (i) vliv mzdy na tendenci úředníků přijímat úplatky je negativní a že (ii) účinek mzdy na náklonnost úředníků recipročně jednat (tj. oplatit úplatek poskytnutím korupční služby) je ve tvaru U.

Ve třetí kapitole navrhuji vysvětlení tohoto pozitivního vztahu ve tvaru písmena U, který byl nalezen v předchozím výzkumu: Když mzda úředníka stoupne nad určitý limit, úředníkův vnímaný sociálně-ekonomický status (SES) stoupne a on se stane méně averzní vůči korupci. Tuto teorii zkoumám za pomoci laboratorního experimentu, ve kterém mohou úředníci přijmout úplatek a poté se rozhodnout, zda budou recipročně jednat. Toto jednání prospívá úplatkáři, ale vytváří negativní externality na sociální blaho. Experimentálně měním SES úředníků, abych otestovala, zda takový efekt existuje, a pokud ano, tak kdy nastane. Výsledky uvedené v této práci naznačují, že vliv SES na sklon úředníků s nejvyšší mzdou přijmout a oplatit úplatky je pozitivní a významný pokud je úplatek dostatečně vysoký.

Introduction

Corruption remains one of the most serious obstacles to the economic and social development of countries: It contributes to high levels of poverty and low levels of investment, GDP and institutional quality, as well as low-level flows of capital, goods, and aid (Cieślik and Goczek (2018), Treisman (2000), and Mauro (1995) among others). Although the determinants of corruption are increasingly understood, we continue to observe it at high levels in the majority of countries worldwide[1]. This could suggest that, apart from the political difficulties of implementing anti-corruption programs, there are determinants of corruption that are not yet well enough understood. It is likely that current anticorruption programs are informed by studies that do not go far enough in investigating the causes and means of fighting corruption.

Most existing studies on corruption tend to search for its determinants on a macro- level. Cross-country comparisons have concluded that the main determinants of corruption are, above all, the absence of democracy (Lambsdorff, 2006; Treisman, 2000), the presence of discretionary power and economic rents associated with that power (Tanzi, 1994; Ades and Di Tella, 1999), and the failure of the judicial system and institutions (La Porta, Lopez-de-Silanes, Shleifer, and Vishny, 1999). Investigating the micro-level (individual-level) determinants of corruption, however, may provide a deeper understanding of the factors that prompt people to engage in corrupt behavior, and consequently, of its persistence (Gatti, Paternostro, and Rigolini, 2003; Dusek, Ortmann and Lizal, 2005).

In the present study I focus on the microeconomic causes of corruption. In particular, in the first chapter, I investigate the peer effect of corruption and the main sources of its formation, using an experimental approach. I use an adapted version of the "take"- treatment of the public goods experiment described by Dufwenberg, Gächter, and Henning-Schmidt (2011) for this purpose. Experimental participants have the opportunity to withdraw money from a common pool. Withdrawal of money benefits the subject but imposes small externalities on each member of the group. Before they make their withdrawal decision, I manipulate subjects' beliefs about the

[1] http://www.transparency.org/cpi2014

behavior of other participants. I also investigate which type of expectations, empirical or normative, most influences the choice to adopt corrupt behavior. I also introduce the probability of being caught and punished for behaving in a corrupt manner, which adds to the negative connotation of withdrawing money from the common pool. I argue that the design of my experiment is more suitable for studying corrupt behavior, and thus the peer effect of corruption, than the two experiments on the peer effects of anti-social behavior that relate most closely to my study (i.e., Bicchieri and Xiao, 2009; Innes and Mitra, 2013). My experiment also allows separate investigation of the decision on how much to withdraw from the common pool (the level of one's corruptibility) from the decision on whether to withdraw or not (decision to behave corruptly). I also use the method described by Bicchieri and Xiao (2009) to learn which type of expectations, empirical or normative, most influence the peer effect. I find that the direction of the peer effect differs depending on what is studied, the level of corruptibility or the decision to behave corruptly. Providing information about others' low level of withdrawal significantly reduces the level of corruptibility (withdrawal) but does not seem to affect the decision to behave corruptly. Providing information about others' high level of withdrawal, on the other hand, does not affect the level of corruptibility (withdrawal) but significantly increases the propensity to behave corruptly. Empirical expectations are found to be the main source of the peer effect of corruption in my experiment.

In the second chapter, I use an experimental approach to examine the link between a public official's wage and his/her corruptibility. The participants in my experiment, playing the role of public officials, decide whether to accept the bribes offered by firms and then whether to act on them (provide corrupt services). If a bribe is accepted and the corrupt service is provided, the sum of money is deducted from a good cause – a real-life charity that in my study represents social welfare. During the four treatments of the experiment, I manipulated the initial endowment of public officials in order to observe how their propensity to take a bribe and act on it (provide a corrupt service) would change. By allowing for multiple wage levels my experiment enables me to study possible non-monotonicity in the wage-corruption relationship, which has not yet been studied in related experimental studies on this relationship. I find that the dependence between officials' wages and bribe acceptance is linear and negative. The propensity to accept and reciprocate the bribe (to provide a corrupt service), however, seems to be U-shaped: It decreases with relative wage until the latter equals two and starts increasing again beyond this level. These

findings may explain why the majority of experimental studies find this dependence to be negative and linear, while Schulze, Sjahrir, and Zakharov (2016) and Chen and Liu (2018), using hard data, find it to be U-shaped.

In the third chapter, which builds on the second chapter, I investigate a possible explanation of the positive, and U-shaped, wage-corruption relationship found in the previous research. In particular, I study if, as highly-paid officials' self-perceived socio-economic status (SSES) increases they become less averse to corrupt behavior. I manipulate officials' SSES experimentally to test if there is some such effect and when it kicks in. I argue that my experimental approach improves on previous research in several ways and thus tends to provide more reliable results. Unlike theoretical or empirical studies on corruption that use hard data, I observe the effect of the increase in SSES on corruptibility in a controlled environment that is relatively free of confounding factors. Unlike previous experimental studies on the relationship between SSES and anti-social behavior, e.g. Piff, Stancato, Cote, Mendoza-Denton, and Keltner (2012), I use an experimental design that is more in line with the experimental practices of experimental economists (e.g., proper incentivization, no deception; see Hertwig and Ortmann 2008). The results reported in this paper suggest that the effect of SSES on the propensity of officials with the highest wage to accept and reciprocate bribes is positive and significant if a bribe is high enough, but not significant otherwise.

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Chapter 1

Investigating the Peer Effect of Anti-Social (Corrupt) Behavior[2]

Abstract

When an individual believes that the majority of his/her peers behave in a corrupt manner, is he/she more likely to behave corruptly? If so, then what motivates an individual to follow the behavior of others more: knowing that the majority behaves in a corrupt manner or the willingness to fulfill the expectations of one's peers? I use a one-shot reverse public goods experiment to provide answers to these questions. In the experiment, subjects have an opportunity to withdraw money from a common pool. Withdrawal of money benefits the subject but imposes small externalities on each member of the group. Before they make their withdrawal decision, I manipulate subjects' beliefs about the behavior of other participants. I also investigate which type of expectation, empirical or normative, has greater influence on the choice to adopt corrupt behavior. The results suggest the existence of a peer effect of corrupt behavior. Expectations of one's peers are found to be the main source of anti-social behavior of subjects.

Keywords: social norms; corruption; anti-social behavior; peer effect; public goods game
JEL classification: C92, D83, D84, K420

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1.1 Introduction

People's decisions and behavior are often informed by societal influences. These influences can either be forced, e.g. by laws, or voluntarily accepted through, for example, social norms and peer effects (Salmon and Serra, 2017). The latter have been shown to induce individuals to adopt illegal or anti-social behavior, thus breaking law(s) and/or social norms. Consequently, studying peer effects and their origins is recognized as important for understanding individuals' behavior and the conditions for successful collective actions (Fehr and Fischbacher (2002), among others).

In this paper I study the peer effects of anti-social, in particular corrupt, behavior (from now on the peer effect of corruption). 'Peer effect' can be defined as the tendencies of individuals to behave in the same way as other members of a group to which they belong (Tirole, 1996). According to Gino, Ayal, and Ariely (2009), peers can influence other individuals' behavior in three ways: through a change in the expected probability of being caught and punished; through a change in perception of saliency of ethicality when considering whether to behave corruptly or not; and through a change in understanding of the social norms related to corruptibility. In the present paper I focus on the last-mentioned source of peer effect, and investigate whether the change in understanding of the social norms related to corruptibility, caused by exposure to other people's (un)ethical behavior, influences individuals' decisions to behave corruptly.

While theories abound, and while various empirical studies have investigated corruption and corruptibility, convincing evidence of the importance of the peer effect of corruption is missing. The difficulty of separating the pure effects of peers' behavior from the effects of confounding factors is the main obstacle faced by studies that use observational data (i.e. Gatti, Paternostro, and Rigolini, 2003; and Dong, Dulleck, and Torgler; 2012). Consequently, the results of such studies are potentially afflicted by fixed-effects, self-selection, e.g. into neighborhoods, and/or measurement error problems (Falk and Ichino, 2006; Falk and Fischbacher, 2002). To address these limitations, some economists have used experiments to study the peer effect of corruption in order to improve upon the existing research. There are several compelling reasons to be interested in the experimental approach for this purpose. One of them is the opportunity to create an appropriate environment and manipulate it to observe the subsequent changes in individuals' behavior. This addresses the endogeneity problem of studies using observational data

(Dusek, Ortmann and Lizal, 2005; Abbink, 2006). For the external validity of the results, however, it is desirable that the experimental design closely relates to the real-life version of the issue that it studies.

In the present study I use an experimental design that is suitable for studying corrupt behavior. However, to understand why it is so, it is important to define corruption before describing the design. According to Transparency International, corruption is broadly defined as the abuse of power for private gain usually at the expense of the public good[3] (Transparency International, 2018). An official using his position at the office to steal money from the funds allocated to poverty-relief is an example of corruption: Stealing money increases her income, but imposes negative externalities on the society as less money is distributed among the poor[4] and the society suffers from the increased crime rate associated with poverty. In this paper, I use an adapted version of the "take"- treatment of the public goods experiment (from now on the reverse public goods game) described by Dufwenberg, Gächter, and Henning-Schmidt (2011). Experimental participants have the opportunity to withdraw money from a common pool. The withdrawal of money benefits the participant but imposes small negative externalities on each member of the group. Thus, the reverse public goods game fits the definition of corruption and serves the purpose of the experiment. Unlike existing experiments on the peer effects of anti-social behavior (e.g., Bicchieri and Xiao, 2009; Innes and Mitra, 2013), I also introduce the probability of being caught and punished for behaving in a corrupt manner, which intensifies the negative connotation of withdrawing money from the common pool. In order to reveal the peer effect of corruption, I manipulate, in two experimental treatments, the participants' beliefs about the behavior of other members of the group to which they belong, and observe the effect of the information provided on subjects' decisions to withdraw money from a common pool.

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[3] Some argue that corruption can also have positive effects as a means of lessening the costs of unnecessary bureaucratic procedures such as jumping a queue or speeding up the process of getting licenses etc. Pope (2000), however, argues that even these "grease payments" eventually impose negative externalities on the society, since they cause decisions to be weighed in terms of money, not human need. Also, any type of corruption leads to a decline in officials' morale, thus "eroding the courage necessary to adhere to high standards of probity" (p.6). Consequently, having once been given a "grease payment", an official may tend to demand the same payment from other people. In Ukraine, for example, one can often observe that certain bureaucratic procedures (like getting a passport) are deliberately prolonged in order to receive bribes from those who cannot wait that long.

[4] Pope (2000), when citing the report of the National Audit Bureau in China, claims that about one fifth of funds allocated to alleviate poverty are diverted into private accounts by the government.

In my experiment, I use the method of Bicchieri and Xiao (2009) to show which source is more likely to trigger a peer effect: beliefs about what most people choose when they decide on whether to behave corruptly or not (empirical expectations), or beliefs about which type of behavior, corrupt or non-corrupt, most people approve of in a particular situation (normative expectations). The relationship between these two kinds of expectations depends on the level of corruption in a country. In spite of an existing social norm condemning corruption, frequent exposure to instances of corruption in countries where it is at a high level leads to inconsistency between the normative and empirical expectations of their citizens. On the other hand, when the level of corruption in a country is low and the non-corruption social norm is largely followed, the direction of the two types of expectations tends to coincide (Bicchieri and Xiao, 2009). In my experiment, in order to check for the empirical and normative expectations I asked subjects a set of questions, the answers to which revealed their personal attitude and beliefs about their peers' attitude to money withdrawal, as well as beliefs about choices made by their peers in the experiment.

Overall, the findings suggest the existence of a peer effect of corruption. Empirical expectations are found to be the main source of peer effects in the experiment, but normative expectations also appear to influence individuals' behavior. These findings could be useful for policymakers in charge of anti-corruption campaigns. For instance, policymakers could better communicate to the general public instances of positive anti-corruption behavior. In the following sections I describe the experiment and its results in detail.

1.2 Related literature

Many existing studies investigate the possibility that peer effects moderate individuals' corrupt behavior. López-Valcárcel, Jiménez and Perdiguero (2017), for example, use the data on all accusations of municipal corruption in Spanish municipalities from 2000 to 2011 and uncover spatial patterns in the distribution of corruption between these municipalities. The authors thus conclude that local corruption is contagious. Bo, Chang and Chun-Ping (2018) employ data from

the World Development Indicators (WDI) and Worldwide Governance Indicators (WGI) databases on corruption perceptions for 109 countries from 2002 to 2013 and also find the presence of a positive relationship between the levels of corruption of neighboring countries. Another study by Gatti et al. (2003) investigates the peer effect of corruption using micro-level data, which were collected using a questionnaire on individual attitudes towards corruption. The authors find evidence that the social environment significantly affects the individual's willingness to behave corruptly. While insightful, the results of these studies are not persuasive for several reasons. First, there is the difficulty of separating the peer effect from the effects of confounding factors discussed above. Second, like other empirical papers on corruption, these studies may suffer from reverse causality/endogeneity and measurement error problems. The issue of reverse causality may arise as some consequences of corruption are difficult to distinguish from its causes (Lambsdorff, 2006). Inequality in a country, for example, can explain high levels of corruption, but econometric evidence also exists of the effect of high levels of corruption on inequality. The measurement error problem may arise due to problems with collecting data on corruption (Dusek et al., 2005). Thus, empirical studies of corruption based on questionnaires and case studies (e.g. Gatti et al., 2003) may not be reliable enough, due to the fact that people tend to lie about their corrupt activity, thus biasing the results. The reliability of the results of the studies using data on corruption perceptions (e.g. Bo et al., 2018) may suffer due to potential differences between corruption perceptions and the real level of corruption (Schulze, Sjahrir, and Zakharov (2016) and Donchev and Ujhelyi, 2014). Finally, the studies using the number of cases of corruption as a proxy for corruption (e.g. López-Valcárcel et al., 2017) may be biased as they only consider the cases investigated by the police. Additionally, all three of the studies mentioned investigate the peer effect of corruption on a macro or aggregated level, while in order to study the reasons behind an individual's choice to behave corruptly, as is intended in this study, one needs to focus on individual-level data.

Bicchieri and Xiao (2009) and Innes and Mitra (2013) take advantage of the experimental approach to improve upon the possible drawbacks of the studies using observational data on the peer effect of unethical behavior, and use corruption as an example of such behavior. The design of both studies, however, may not be suitable for my purpose: Bicchieri and Xiao (2009) use the dictator game and Innes and Mitra (2013) use the deception game. Neither of these games, however, take into account the social loss created by corrupt behavior. The existence of such a loss may influence an individual's decision to be involved in corrupt behavior (Andreoni, 1995; Barr

and Serra, 2009). Thus, the peer effects of selfishness/unfairness, which is the focus of the dictator game, or dishonesty, which is studied in the deception game, and corruptibility, may have different mechanisms behind them. Moreover, selfishness, dishonesty, and corruptibility are different concepts. According to Innes and Mitra (2013), one of the interpretations of selfishness is the willingness to compete and win. Selfishness is not always perceived as negative: It can promote innovations and development. Unlike corruption, it is also not illegal. Dishonesty is also not illegal but is perceived by church and society as wrongdoing and a violation of a social norm. In this respect, corruptibility is different from selfishness and dishonesty: corruption is illegal and the way people perceive it depends on the overall level of corruption in a country, i.e.in a highly corrupted society, people tend to perceive taking or giving bribes as the normal everyday behavior of making deals (Cabelkova, 2001). In such a society being corrupt is not perceived in the same way as being dishonest or selfish. Therefore, the peer effect of dishonest or selfish behavior may differ from the peer effect of corruption, and a different experimental game should be used to study it.

Finally, a recent experiment of Abbink, Freidin, Gangadharan and Moro (2018) on the relationship between culture and corruption employs a bribery game to find out if participants in the role of firms offer more bribes when paired with officials from a corrupt group versus from an honest group (the groups of officials were formed based on the results of a separate treatment). The authors find that firms offered twice as many bribes to officials from the corrupt group than to their "honest" colleagues. This result, however, may not be applicable when the peer effect of corruption is considered. Firms and officials in the bribery game clearly play different roles and firms may not consider them as "peers".

1.3 Theoretical background of the experiment

The reverse public goods game described in Dufwenberg et al. (2011) is the point of departure for my experiment. In this game, a group of subjects is endowed with resources (T) and each member of the group is allowed to withdraw money from this common pool up to a certain amount. After all participants have made a decision about whether to withdraw money and how much, the money remaining in the account is multiplied by a constant μ and distributed equally between all members in the group. The payoff function of participant i (π_i) is given by

$$\pi_i = t_i + [\mu \times (T - t_1 - t_2 \ldots - t_i - t_j - t_n)] \div n,$$

where $t_i \in \{0,1,\ldots,T/n\}$ is the amount of money each subject i can take from the public account, i= 1, 2, …n, and n is the total number of participants in a group.

The negative externalities imposed by participant j into participant i, E_{ji}, are calculated as a difference between the maximum possible payoff of participant i[5], π_{i0}, and the payoff of participant i when participant j withdraws t_j from the common pool (π_{ij}):

$$E_{ji} = \pi_{i0} - \pi_{ij} = t_i + [\mu \times (T - t_i)] \div n - [t_i + [\mu \times (T - t_i - t_j)] \div n] = \frac{\mu}{n} \times t_j .$$

In order to trigger the sentiment that participants' anti-social behavior has negative consequences to other members of the group, a probability of being caught and punished is introduced into the game: Once a participant decides to withdraw money she can lose all her payoff with positive probability p<1.

If all the participants are selfish money-maximizers, the sub-game perfect equilibrium of this game is as follows: each participant will withdraw the maximum possible amount of money from the common pool, so each 'other member of the group' will suffer the maximum possible negative externality of $\frac{\mu}{n} \times T$.

1.4 Experimental design and implementation

[5] The payoff of participant *i* is maximized when *i* withdraws the maximum possible t_i and *j* does not withdraw anything (t_j=0).

In the experiment, subjects were randomly assigned to groups of six people. Each group was endowed with 1500 Talers (experimental units). The maximum amount of money that each subject was allowed to withdraw was 250 Talers, the constant multiplier μ was equal to 3[6] and the probability of being caught and punished p was set to 0.003[7]. Thus, the payoff function was given by

$$\pi_i = t_i + [3 \times (1500 - \sum_{j=1}^{6} t_j)] \div 6$$

The experiment consisted of three main treatments: Control treatment (Control), Positive information treatment (PosInfo) and Negative information treatment (NegInfo). The PosInfo and NegInfo treatments were intended to find out the peer effect of money withdrawal. During these treatments, the participants were provided with information about the choices of the participants in the two previous consecutive sessions before having to make a decision about how much to withdraw.

1.4.1 Providing information about others' behavior

Providing information about others' behavior differed from the methods used in existing studies and thus I describe it in detail. To my knowledge, two main ways of providing the information aimed at manipulating subjects' beliefs have been widely used in the studies on the topic: First, providing untrue information (i.e., Fleishman, 1988) or providing information about a nonrandom sample of individuals and presenting it as if it describes a random sample. Innes and Mitra (2013), for example provided the information about the choices made by a group of

[6] The parameters repeat those used in Dufwenberg et al. (2011) but are changed with respect to the larger number of members in the group and the exchange rate between the Czech Koruna and Euro (1 Euro= 25 Czk). With these parameters the marginal per capita return and the average payoff of the players are similar to those in the experiment of Dufwenberg et al. (2011)

[7] The probability of being caught and punished follows Abbink, Irlenbusch and Renner (2002). In their paper, the authors use a repeated game, thus making the total probability of being caught for repeat offenders higher. But for one-time offenders (even in a repeated game) the probability stays equal to 0.003. So, in the present paper I decided to keep it as such, as the main goal of introducing it was to carry a message of immorality of money withdrawal. This is a matter for future research to check if increasing this probability changes the results of the experiment.

participants selected from different experimental sessions who behaved in a highly corrupt manner for the negative-information treatment, and for a group of those who behaved in a non-corrupt manner for the positive-information treatment. Similarly, Bicchieri and Xiao (2009), in their dictator game, provided information about the behavior of all participants but selected the session which satisfied the required criteria: either a very fair or very unfair session. The participants in the experimental treatment, however, were not told that the sample of participants or the session was selected non-randomly. Thus the statements used in these studies seem to suggest that the reported decisions represent a general pattern. Intentional provision of misinformation, however, is deception, and according to Hertwig and Ortmann (2008), it tends to "raise participants' suspicions, prompt second-guessing of the experimenters' true intentions, and ultimately distorts behavior and endangers the [experimental] control it is meant to achieve" (p.59).

The second way to provide information about the behavior of "peers" is to use the strategy method of Selten (1967). The strategy method implies that subjects should state what they would do in hypothetical situations often in response to other participants' hypothetical actions (in my case, if other participants withdrew a particular amount of money from the common pool). This method avoids deception of participants and helps to collect data at a relatively low cost. However, due to its hypothetical nature the strategy method has been strongly criticized for being too psychologically cold to be realistic and externally valid (Brandts and Charness, 2000). Brandts and Charness (2000) suggest that some actions would "trigger stronger emotional responses in a hot direct-response environment" (p.228) (when the second player responds to the first player's observed action) than when the strategy method is used. Roth (1995) points out that this difference in behavior may occur due to the fact that for some decision-making processes timing is an important aspect which can influence a subject's decision. Since under the strategy method subjects have to specify their behavior in advance, the possibility of observing this timing effect is removed. Hence, in some experiments, there can be a difference in participants' behavior in the "hot" and "cold" conditions[8].

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[8] The experimental evidence on whether the cold- and hot-response methods give different results is mixed (for a review see Brandts and Charness, 2011). A number of studies do find a difference between the treatment effects obtained while using the hypothetical-response and direct- response methods. Brandts and Charness (2003), for example, find that in a modified version of a deception game with a possibility to punish, the level of punishment doubles in the "hot" condition. Similarly, Brosin, Weimann and Yang (2003) discover that when the costs of punishment of unfavorable behavior of a partner are relatively low, the probability of being punished in a direct

For the reasons already stated, I decided to avoid using the two methods described above and provided information about others' behavior as follows: Before answering how much they want to withdraw, the participants in the PosInfo treatment received information about the six (out of twenty-four) smallest withdrawals in the two consecutive previous sessions, while the subjects in the NegInfo treatment received information about the six (out of twenty-four) highest withdrawals in the two consecutive previous sessions [9]. This approach has two distinct advantages compared to the intentional provision of misinformation and the strategy method. First, unlike the former it provides true information about others' behavior without misrepresentation of the take-pattern of the whole group. Second, this method provides information to participants about real choices made by the participants who are similar to them. This creates a "hot" (a direct-response) environment, which tends to trigger stronger emotional responses than when the strategy method is used. In order to perform the robustness check, however, an additional "cold" treatment based on an adapted version of Selten's strategy method[10] ("cold-response"-method treatment) was conducted, where I elicited subjects' conditional willingness to withdraw the money from the common pool. In particular, the participants in the "cold-response"-method treatment faced two alternative hypothetical situations: In the first situation, before answering how much they wanted to withdraw the participants were asked to imagine that the six smallest withdrawals of the twenty four participants in two previous sessions were 0,0,0,0,0,0 Talers (ColdNeg group). In the second situation, the participants were asked to imagine that the six largest withdrawals of the twenty four participants in two previous sessions were 250,250,250,250,250,250 Talers (ColdNeg group). In each session, I changed the order of the situations the participants faced to balance out the effect the order might have had on participants' decisions. The results show that the behavior of subjects

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response game is 42% while in the hypothetical-response method game it is 0%. Also, Casari and Cason (2009) find significant difference in the behavior of participants in the "hot" and "cold" treatments in the trust game. These findings question the hypothesis that the hypothetical-response method always gives the same results as the direct response.

[9] This information was true because in each of the two consecutive sessions at least six out of twenty four subjects either withdrew nothing or withdrew the maximum possible amount.

[10] There are important differences between how the strategy method is usually applied and how I use it in this paper. The strategy method is generally used to elicit the strategy space for later movers in multi-stage games and is thus not applicable in my simultaneous-move one-shot game. I use an adapted version of the strategy method, which aims to elicit later movers' strategies as a response to hypothetical choices made by the first movers. I use the term "cold-response" method to describe the method that I use.

differs in “hot” and “cold” treatments, but the difference is not statistically significant for the setting.

1.4.2 Eliciting empirical and normative expectations

In order to distinguish which kind of expectations, empirical or normative, influence the decision to withdraw money, I followed Bicchieri and Xiao (2009) and asked the subjects to answer several questions. The answers to the first question helped to elicit empirical expectations: “Please estimate how many members of your group including you have taken Talers from the common pool” (from now on emex_members). I also asked the second question to see how the subject's belief about an average amount of money withdrawn from the common pool influenced her behavior: “Please estimate the average amount of Talers withdrawn from the common pool by all members of your group” (emex_amount). The answers to the third and fourth questions gave us information about the individual's normative expectations. The subjects were asked: “Do you think it is appropriate to withdraw money from the common pool?” and then “Please estimate how many members of your group including you answered positively to the previous question” (henceforth noex). Then, during the analysis of the results, I checked if the answers to the first, second and the fourth questions influenced the amount withdrawn by the subject from the common pool. To ensure that the answers to the questions were thought through, subjects were paid a reward of 100 Talers if his/her answer matched the actual number[11]. All questions were asked after the subjects made their decisions about the withdrawal. Nobody was informed about the subsequent questions before making decisions[12].

The participants in the fourth, "cold-response"-method, treatment were asked the same four questions but before answering them, they were faced with two alternative hypothetical situations: In the first situation, the participants were asked to imagine that the six smallest withdrawals of

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[11] In the case when the subjects were asked to estimate the average amount of Talers withdrawn from the common pool they were paid 100 Talers if their estimates were correct, with a toleration of 13 Talers.
[12] Dufwenberg et al.(2011) assert that the different timing of events could induce the participants to make a choice that they think would lead to the correct prediction.

the twenty-four participants in two previous sessions were 0,0,0,0,0,0 Talers. In the second situation, the participants were asked to imagine that the six largest withdrawals of the twenty-four participants in two previous sessions were 250,250,250,250,250,250 Talers. Again, in each session, I changed the order of the situations the participants faced to balance out the effect the order might have had on participants' decisions. At the end of the experiment, the participants in the "cold-response"-method treatment were paid their payoffs for only one of the two situations if the answer matched the actual number. The computer randomly determined which of the two situations was realized and paid out. The subjects were then notified.

1.4.3 Model and hypotheses

In the experiment, the level of corruption among the participants was measured by the amount of money withdrawn from the common pool. This outcome variable was used to test the following research hypotheses.

Hypothesis 1 (H1): Providing information about peers' non-corrupt behavior does not affect the level of corruption among individuals.

Hypothesis 2 (H2): Providing information about peers' corrupt behavior does not affect the level of corruption among individuals.

Hypothesis 3 (H3): Empirical expectations do not affect the level of corruption among individuals.

Hypothesis 4 (H4): Normative expectations do not affect the level of corruption among individuals.

Hypothesis 5 (H5): There is no difference between subjects' corrupt behavior under "hot" and "cold" treatments.

At this point it is worth mentioning that although the group's total payoff is maximized when no money is withdrawn from the common pool, the subgame-perfect Nash equilibrium of the staged game is for a rational (in the sense of selfish) participant to always withdraw the maximum amount of money regardless of whatever anyone else does.

1.4.4 Experimental settings and payoffs

The experiment used a between-subject design, with each subject participating in one treatment only. This was to ensure that the choices were strictly independent. All experimental sessions were run by myself at the Laboratory of Experimental Economics (LEE) at the University of Economics in Prague between November14-24 2014 using zTree software (Fischbacher 2007). Subjects were recruited through the Online Recruitment System for Economic Experiments (Greiner, 2004).

In total, I recruited 180 subjects for the data collection: 144 subjects (12 sessions of 12 subjects) for the three main treatments and 36 (3 sessions of 12 subjects) additional subjects for the "cold-response"-method treatment. The age of the participants ranged from 18 to 40 (with a median of 23) and the largest group (41%) mentioned Economics (or Mathematical Economics) as their major.

Before entering the laboratory, the participants were randomly assigned to their seats. The instructions for the experiments (see Appendix 1 A) consisted of two parts. The first part of the instructions with the explanation of the rules, game and payoffs was given to the participants immediately and in paper form. Thus, participants could refer to the instructions whenever they needed. The second part of the instructions informed the participants about the emex and noex questionnaire (see Appendix 1 C). It was shown on the screen of the computers.

Participation in the experiment was rewarded by cash at the end of the experiment and was dependent on performance. The payoffs from the reverse public goods game were transferred into Czech Koruna using the exchange rate of 3Taler = 1 CZK. Apart from that, subjects were paid a show-up fee of 100 CZK (around 3.62 Euro). The average subject cash payoff was 290 CZK (around 10.49 Euro) including the show-up fee. This was the average compensation for about one

hour of net participation time. Three subjects (two subjects from the PosInfo and one from the NegInfo treatments) were "caught"[13] for withdrawing money and were only paid the show-up fee.

1.5 Results

The data analysis consists of two parts. First, I examine how the information about peers' behavior affects subjects' decisions to withdraw money from the common pool (H1, H2 and H5). Second, I analyze how the empirical and normative expectations affect the behavior of the participants (H3 and H4). During the analysis I separately study the effects of the treatment manipulations of subjects' beliefs about their peers' behavior on the level of withdrawal and on the propensity to withdraw any positive amount from the common pool. I also check how "hot" and "cold" treatment conditions affect subjects' behavior. In Section 1.6 I provide a possible explanation for the results obtained.

1.5.1 Decision to withdraw

Result 1. Providing the information about others' low level of withdrawal reduces the amount of money withdrawn from the common pool (reject H1). Providing the information about others' high level of withdrawal does not affect the level of withdrawal (cannot reject H2).

Result 2: There is no significant difference in subjects' levels of withdrawal in "hot" and "cold" treatments (cannot reject H5).

[13] As the probability of being caught and punished was set equal to 0.003, the computer randomly assigned a number from 1 to 1000 to each participant in a session. Those whose number was either 1.2 or 3 and who withdrew a positive amount from the common pool were "caught" and punished.

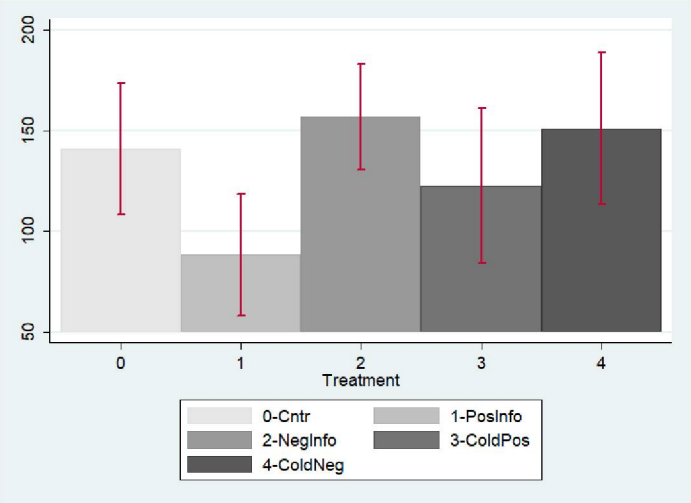


Figure 1.1: Mean withdrawals over treatments.

Table 1.1: Mean and median withdrawals over treatments

Treatment	Mean withdrawal	Median withdrawal
Control[14]	141	200
PosInfo	88	25
NegInfo	156	165
ColdNeg	122	105
ColdNeg	151	205

Table 1.2: P-values of pair-wise Wilcoxon rank sum tests comparing mean withdrawals over main treatments.

	Control	NegInfo	PosInfo
Control		0.6377	0.0755
Observations	48	48	48

Table 1.3: Results of a robust regression for amount withdrawn on treatment dummies.

Dependent variable: Amount withdrawn	
	(1)
PosInfo	-58.98***

[14] Variable descriptions and statistics are provided in Appendix 1 D.

	(22.39)
NegInfo	11.20
	(21.84)
Male	34.51**
	(18.49)
Age	2.74
	(2.61)
Econ student[15]	13.10
	(18.69)
Constant	57.94
	(63.60)
Observations	144

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Fig. 1.1, Table 1.1 and Table 1.2 provide the main support for Result 1 and Result 2. The mean/median withdrawals are summarized in Table 1.1 and Fig. 1.1. A simple comparison of the mean withdrawals across treatments suggests that the difference between withdrawals tends to be significant only for the PosInfo treatment. We check if this is true using non-parametric and parametric tests. A non-parametric Kruskal-Wallis test suggests that withdrawals from the three main treatments (Control, PosInfo and NegInfo) stem from different distributions ($\chi^2(2) = 7.984$; $p = 0.0185$). Pair-wise Wilcoxon rank sum tests for a pair-wise treatment comparison, however, suggest that only withdrawals from the Control and Positive information treatments stem from different distributions (p-values are documented in Table 1.2). This suggests that only the positive-information manipulation was effective enough in facilitating a change in the amount of money withdrawn by the subjects.

As for the direction of the effect, a robust regression of withdrawals on dummies for treatments indicates that providing positive information decreases the amount withdrawn, while providing negative information increases it (Table 1.3). This last effect, however, is not statistically significant.

Comparison of the withdrawals in Positive and Negative information treatments with those in the "cold-response"-method treatment showed that using the "cold-response" method in the one-

[15] The original variable for which the data were collected was the participant's major. A new variable, Econ_student, is a dummy variable which equals one if a participant chose either Economics or Economic mathematics as her major. Some observations for this variable are missing for sessions 1 and 2, but as this is not the main variable of interest I chose to impute zeros instead of these missing values. Nevertheless, an alternative solution, regressing without a major-dummy, gives very similar results.

shot reverse public-goods experiment tends to induce weaker responses from subjects than the direct- response method, but this difference is statistically insignificant. In particular, pair-wise

Wilcoxon rank sum tests cannot reject the hypothesis that the withdrawals from the PosInfo and ColdNeg group of the "cold-response"-method treatment, and the NegInfo and ColdNeg group of the "cold-response"-method treatment, come from the same distribution: p-values are 0.2484 and 0.7751, respectively.

I also check if there is a peer effect on the propensity of subjects to withdraw any positive amount of money from the common pool (to become corrupt) which leads to Result 3.

Result 3. Providing the information about others' low level of withdrawal does not affect the propensity to withdraw. Providing the information about others' high level of withdrawal increases the propensity to withdraw.

Table 1.4 provides the main support for Result 3. In the table, I present the results of a probit regression with the binary dependent variable being equal to one if a subject's withdrawal is positive and zero otherwise. The independent variables include dummies for treatments and personal characteristics. The results show that providing negative information about peers' behavior increases the propensity of subjects to withdraw a positive amount from the common

Table 1.4: Results of a probit regression for the decision to withdraw on treatment dummies.

Dependent variable: Money is withdrawn (1 = yes)	
	(1)
PosInfo	-.10
	(.09)
NegInfo	.18*
	(.10)
Male	.06
	(.08)
Age	.003
	(.02)
Econ student	.01
	(.08)
Observations	144

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0

pool and this effect is statistically significant. Providing positive information, on the other hand, decreases subjects' propensity to withdraw a positive amount of money from the common pool.

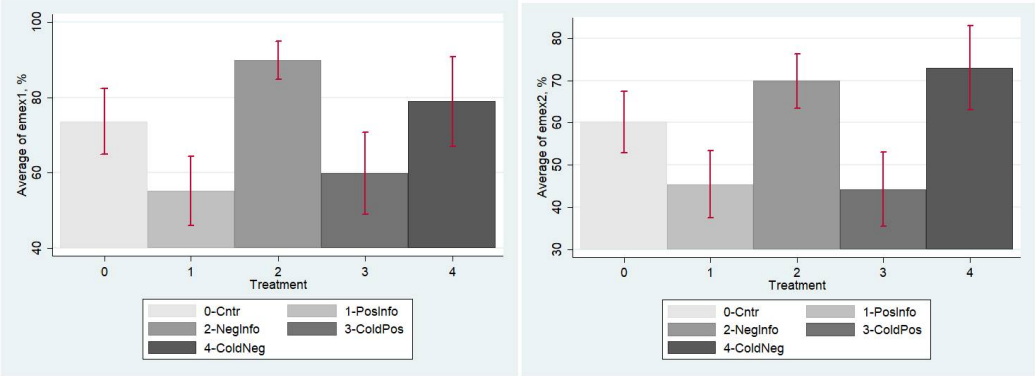
Result 4. There is no significant difference in subjects' propensity to withdraw in "hot" and "cold" treatments.

The results of two probit regressions with the dependent variable being equal to one if a subject's withdrawal is positive and zero otherwise and independent variables – dummy for the ColdNeg (model 1) or ColdNeg (model 2) group and other personal characteristics provide the support for Result 4[16]. The coefficients before the main variables of interest, dummies for the ColdNeg and ColdNeg are negative but not statistically significant (coefficient of (-0.02) and p-value 0.11in model 1 and coefficient of (-0.17) and p-value 0.09 in model 2).

1.5.2 Empirical and normative expectations

From the emex_members, emex_amount and noex I calculate the percentage of group members who a participant i believed withdrew money from the common pool (in the case of emex_members), money she believed was withdrawn from the common pool (in the case of emex_amount) and participants she believed thought withdrawal was appropriate (in the case of noex). When analyzing emex_members, I drop four observations from the ColdNeg group and five from the ColdNeg group as their answers to the question eliciting emex_members in percent exceeded 100%. I use these newly obtained variables (emex_members_perc, emex_amount_perc and noex_perc) to arrive at Result 5.

[16] These probit regressions were run using data from the PosInfo and StratPos (model 1) and the NegInfo and StratNeg (model 2) only.



(a) (b)

Figure 1.2: Mean of empirical expectations over treatments, percentage: (a) emex_members_perc; (b) emex_amount_perc.

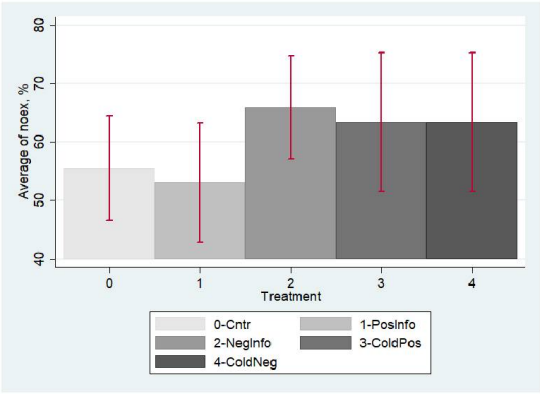


Figure 1.3: Mean of normative expectations (noex_perc) over treatments, percentage.

Table 1.5: Mean and median empirical and normative expectations (percent) over treatments.

Variable		Control	PosInfo	NegInfo	StrPos	StrNeg
emex_members_perc	Mean	73.61	55.21	89.93	59.90	79.03
	Median	83.33	50	100	50	100
	Observations	48	48	48	32	31
emex_amount_perc	Mean	60.17	45.41	69.86	44.22	73.02
	Median	60	40	68.2	40	80
	Observations	48	48	48	36	36
noex_perc	Mean	55.56	53.13	65.97	63.43	63.43
	Median	50	66.67	66.67	66.67	66.67
	Observations	48	48	48	36	36

Result 5. Empirical expectations are the main source of the peer effect of money withdrawal.

This result is supported by the following analysis. A simple comparison of means (Fig. 1.2, Fig. 1.3 and Table 1.5) suggests that empirical expectations (emex_members) drive the change in participants' willingness to withdraw money from the common pool in the PosInfo and NegInfo treatments. I test this conjecture using non-parametric and parametric tests.

Table 1.6: The results of a Kruskal-Wallis test for empirical and normative expectations in the three main treatments

Variable	$\chi^2(2)$	p-value
emex_members_perc	32.39	0.0001
emex_amount_perc	20.17	0.0001
noex_perc	3.92	0.1411

Table 1.7: P-values of pair-wise Wilcoxon rank sum tests comparing mean empirical and normative expectations over the three main treatments.

Variable		PosInfo	NegInfo
emex_members_perc	Control	0.0031	0.0025
emex_amount_prec	Control	0.0043	0.0687
noex_perc	Control	0.9733	0.0861
Observations	48	48	48

A non-parametric Kruskal-Wallis test strongly suggests that both emex_members_perc and emex_amount_perc differ (stem from different distributions) in the three main treatments (Table 1.6). This result is confirmed by pair-wise Wilcoxon rank-sum tests for a pair-wise treatment comparison (Table 1.7). Normex_perc, however, are found to be the same (to stem from the same distribution) in the three main treatments (Table 1.6). Wilcoxon pair-wise rank sum tests imply that only normative expectations from the NegInfo treatment stem from a different distribution than those in the Control treatment (Table 1.7). These results suggest that empirical expectations drive the subjects' decisions on the level of withdrawal.

Table 1.8: Results of robust regressions for the amount withdrawn on empirical and normative expectations over treatments.

Dependent variable: Amount withdrawn					
	(1)	(2)	(3)	(4)	(5)
Variables	Control	PosInfo	NegInfo	ColdNeg	ColdNeg
emex_members_perc	2.578***	2.365***	2.521***	3.290***	2.362***
	(0.618)	(0.385)	(0.494)	(0.369)	(0.481)
noex_perc	0.696	0.367	0.25	0.0344	0.322
	(0.555)	(0.339)	(0.432)	(0.346)	(0.538)
Male	61.91**	30.57	27.62	17.25	40.89
	(25.55)	(22.16)	(23.38)	(36.3)	(28.38)
Age	-1.671	9.013	-2.07	0.458	3.456
	(4.442)	(5.95)	(3.191)	(6.095)	(5.518)
Econ_stud	14.81	5.179	7.322	-14.64	4.254
	(43.6)	(19.91)	(21.07)	(31.91)	(33.16)
Constant	-47.02	-268.0**	-3.288	-41.5	-160.8
	(107.7)	(129.5)	(87.32)	(156.4)	(140.1)
Observations	48	48	48	36	36
R-squared	.504	.540	.491	.604	.526

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Results of probit regressions for the decision to withdrawn on empirical and normative expectations over treatments.

Dependent variable: The money is withdrawn (yes=1)					
	(1)	(2)	(3)	(4)	(5)
Variables	Control	PosInfo	NegInfo	ColdNeg	ColdNeg
emex_members_perc	.010***	.012***	.007***	.008***	.006***
	(.002)	(.001)	(.001)	(.002)	(.002)
noex_perc	.000	-.002	.002	.001	.001
	(.002)	(.001)	(.001)	(.002)	(.002)
Male	.137*	.055	-.032	-.080	.027
	(.070)	(.089)	(.078)	(.159)	(.118)
Age	-.010	.005	-.010	.033	.043*
	(.011)	(.023)	(.011)	(.026)	(.022)
Econ_stud	-.135	-.169**	.019	-.020	.154
	(.093)	(.081)	(.060	(.164	(.128)
Observations	48	48	48	32	31

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.10: P-values of pair-wise Wilcoxon rank sum tests comparing mean empirical and normative expectations in "hot" and "cold" treatments.

Variable		PosInfo	NegInfo
emex_members_perc	ColdNeg	0.597	
	ColdNeg		0.141
emex_amount_prec	ColdNeg	0.975	
	ColdNeg		0.235
noex_perc	ColdNeg	0.159	
	ColdNeg		0.875

A regression of amount withdrawn on empirical (emex_members_perc) and normative (noex_perc) expectations provides additional support for this conclusion (Table 1.8). The effect of empirical expectations on withdrawals is positive and highly significant and the effect of normative expectations is insignificant for all treatments. Based on these results[17], I can reject hypothesis H3 but cannot reject hypothesis H4.

Result 6. Empirical expectations are the main source of peer effects of propensity to withdraw.

Result 6 is supported by the results of a probit model with the binary dependent variable being equal to one if a positive amount of money is withdrawn and zero otherwise on empirical (emex_members_perc) and normative (noex_perc) expectations and personal characteristics. The outcomes of the regressions are presented in Table 1.9. The results show that subjects' propensity to withdraw is driven solely by empirical expectations in all treatments. As expected, the higher empirical expectations are associated with the higher propensity to withdraw.

Result 7. There is no difference between empirical and normative expectations under "hot" and "cold" conditions.

[17] It is worth noting that according to the literature on the false consensus effect (e.g. Mullen, Atkins, Champion, Edwards, Hardy, Story, and Vanderklok; 1985), subjects tend to believe that their own response is the most common one. If true, this creates a reverse causality in the proposed model. As I do not have sufficient tools (e.g. an appropriate instrumental variable) to solve this problem, I choose to follow the estimation procedure offered by Bicchieri and Xiao (2009).

Result 7 is supported by the results of Wilcoxon pair-wise rank sum tests which show that the difference is not statistically significant (Table 1.10).

1.6 Discussion and concluding remarks

In general, the results of the reverse public goods experiment confirms the existence of the peer effect of anti-social corruption-like behavior. The design of the experiment makes it possible to conduct a separate analysis for the amount of money withdrawn from the common pool and for the decision to withdraw any positive amount. The latter can be associated with the officials' propensity to become corrupt. Statistical evidence differs in both cases. I find that when the amount of money withdrawn from the common pool is considered, the effect goes only in a positive direction: Knowledge about others' pro-social behavior leads to a decrease in the amount withdrawn from the common pool. This effect originates in both empirical and normative expectations of the subjects. The effect of the latter implies that information on pro-social choices made by peers forces individuals to also think about the morality of the act of money withdrawal, which prompts them to withdraw less. The peer effect of providing negative information also exists but it appears to be statistically insignificant.

When, however, the propensity to withdraw a positive amount is considered, the peer effect goes in the same direction: Subjects tend to withdraw a positive amount of money more often when they obtain information about others' large withdrawals. When faced with the information that peers behave pro-socially, fewer individuals decided to withdraw the money, but this effect is not statistically significant. This finding is in line with the results of the experiment conducted by Innes and Mitra (2013) in the USA, which suggest that a strong signal of dishonesty from ones' peers leads to more dishonesty, and a strong signal of honesty does not lead to a significant increase in individuals' honesty. Their interpretation of this phenomenon is that those who chose to behave anti-socially, thus violating a social norm, are less prone to change their behavior to pro-social when exposed to social opinion. Since my experiment allows the disentangling of the propensity to behave antisocially (propensity to withdraw) from the extent of anti-social behavior (the amount of money withdrawn) I am able to add to this explanation: When faced with social information that their peers behaved pro-socially, subjects do not stop their anti-social behavior but they significantly lower its extent. This result inspires optimism that when exposed to the prevailing non-corrupt behavior of their peers, individuals may reduce their involvement in corruption in

general or in cases which entail large negative consequences for other people, and at some time possibly stop being corrupt at all.

When empirical and normative expectations are considered, the former tend to be the main source of the peer effect of corruption: The effect of empirical expectations is significant and positive in all treatments when either the amount of money withdrawn or the propensity to withdraw are considered. The positive direction of this effect is in line with Gino (2015) who asserts that people behave anti-socially more often when they can justify their behavior. This result was expected, as the experiment was run in the Czech Republic, where corruption is perceived by most people as wrongdoing. It would require more time and a stronger connection to peers than the one the participants experienced during the present one-shot experimental game to change this perception.

My results suggest the following policy recommendation. In order to reduce the level of corruption in a country a policy-maker should first of all concentrate on the formation of people's perceptions about the level of corruption in the country. This measure will help to both reduce the level of corruption among those who are already involved in corrupt activities and prevent "the innocent" from starting to behave corruptly.

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Appendix 1

A. Instructions

Welcome to theexperiment!

General information on the experiment

You are going to participate in an experiment on decision making. If you read the following instructions carefully, you'll be able to earn a considerable amount of money. How much you will earn will depend on your and others' decisions. It is therefore very important that you understand the following instructions.

- Anonymity

All participants decide anonymously, i.e. the other participants will not learn the decision you made during the experiment. To ensure anonymity it is imperative that all participants observe the following rule: During the experiment all communication is prohibited, i.e. you are not allowed to speak or otherwise express yourself. If you have a question, please raise your hand and an experimenter will come to you and answer it. If you violate this rule, you will be dismissed from the experiment and forfeit all payments.

- Payments

The currency used in this experiment is Talers. Your total earnings will first be calculated in Talers. The total amount of Talers that you have earned during the experiment will be converted into CZK at the end of the experiment at an exchange rate of

3Talers = 1 CZK.

You will also receive a show-up fee of 100 CZK. You will be paid your earnings in cash, and privately, upon leaving the room.

The experiment consists of two stages. Onlyone stage, however, is payoff relevant. This means that at the end of the experiment the computer will randomly, and with equal probability, decide whether your earnings will be determined by your choices in Stage 1 or Stage 2.

In the following pages the experiment is described in detail. Once you have read the instructions carefully, we will ask you to answer several comprehension questions.

Stage 1: The Experimental Procedure

In the experiment, you will be a member of a group of 6 participants. There will be five other members in your group. The group will be endowed with money which will be called the common pool.

The experiment consists of only one task. You will have to decide how many Talers you want to take from the common pool and how many Talers you want to leave in the common pool.

- The decision in the experiment

At the beginning of the first stage, there are 1500 Talers in the common pool of your group. Each participant can take up to 250 Talers from the common pool. You will have to decide how many of these 250 Talers you want to take for yourself (and hence how many you want to leave in the common pool). The five other members of your group have to make the same decision. Every Taler that you take from the common pool for yourself will be paid to you, converted by the exchange rate given above, at the end of the experiment.

Talers that are left in the common pool will be multiplied by 3, The resultant number of Talers will be divided equally among all six members of the group. If for instance 100 Talers were left in the common pool, the number of Talers would increase to 300 Talers. This amount would then be divided equally among all six members of the group. Thus every group member would get 300/6 = 50 Talers in addition to what they took for themselves. After all six members of the group have made their decisions about the amounts they take from the common pool the total earnings obtained by each participant are determined.

- The possibility of losing money earned in Stage 1

If you decide to withdraw the money from the common pool, a number out of the range from 0 to 999 is randomly drawn. If the number is 0, 1 or 2, then you are disqualified (the probability of being disqualified is 0.003). That means that the experiment ends for you and all your previous earnings are canceled (at the end of the experiment, you will receive only the show-up fee.). If the randomly drawn number is 3, 4, ..., 998, or 999 (which happens with probability 0.997), the experiment is continued.

- The calculation of your earnings:

The earnings of every member of the group are calculated in the same way. If you are not disqualified, your earnings consist of two parts:

(1) the earnings from Talers taken: the Talers that you take from the common pool for yourself;

(2) the earnings from the common pool. The earnings from the common pool are calculated as follows:

3 × (1500 - sum of all Talers taken from the common pool)/6 =

= (3/6) × (1500 - sum of all Talers taken from the common pool).

That is each non-disqualified group member receives the same earnings from the common pool.

Therefore your total earnings will be earnings from Talers taken + earnings from the common pool:

Total earnings = (earnings from Talers taken) + (3/6) × (1500 - sum of all Talers taken from the common pool)

The income of each group member from the project is calculated in the same way.

Example:

If you take all 250 Talers from the common pool, your "earnings from Talers taken" is 250. At the same time, the total sum of Talers left in the common pool decreases by 250 Talers and each member's "earnings from the common pool" decreases by (3/6)×250=125 Taler.

If you take 100 Talers from the common pool, your "earnings from Talers taken" is 100. At the same time, the total sum of Talers left in the common pool decreases by 100 Talers and each group member's "earnings from the common pool" decreases by (3/6)×100 = 50 Taler.

- How to take your decision

The experiment consists of one period. The input screen that will prompt you for your decision looks as follows:

In the middle of the screen you will find the information that your group consists of 6 members, that there are 1500 Talers in the common pool and that you can withdraw any amount from 0 to 250 Talers. The formula for calculating your Total earnings is in the bottom of the screen in the Help box. You will find a Calculator button above the Help box. You can use the calculator, which appears after pressing this button, to make the calculations if needed.

You make your decision by typing a number between 0 and 250 in the input field. This field can be reached by clicking it with the mouse. After entering your withdrawal you must press the CONTINUE button. Once you have done this, your decision can no longer be revised.

Stage 2

The instructions for Stage 2 will appear on the screen after you finish Stage 1 of the experiment.

Questionnaire

After you finish Stage 2, you will be asked to fill in a questionnaire. The answers you provide in these questionnaires are completely anonymous.

At the end of the experiment you will see an income screen that informs you about your earnings from your decisions made in Stage1 and 2, the information on which stage will be paid for and your Final earnings in CZK.

If you have any questions please ask them now.

B Comprehension questions

Please answer the following:

If each of the six members of the group takes 250 Talers from the common pool, what will be the total earnings of every member of the group?

If each of the six members of the group takes 0 Talers from the common pool, what will be the total earnings of every member of the group?

If you take 250 Talers from the common pool and the rest of the members of the group takes 0 Talers from the common pool, what will be the total earnings of every member of the group?

If you take 100 Talers from the common pool and the rest of the members of the group takes 0 Talers from the common pool, what will be the total earnings of every member of the group?

If one of the members of the group takes 250 Talers from the common pool and the rest of the members including you take 0 Talers from the common pool, what will be the total earnings of every member of the group?

If you take 0 Talers from the common pool and the rest of the members take 250 Talers from the common pool, what will be the total earnings of every member of the group?

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C: Instructions for Stage 2

Eliciting the empirical and normative expectations

[appeared on the screen after a participant had finished Stage 1 of the experiment]

In three main treatments:

To elicit emex_members:

Please estimate how many members of your group including you have taken Talers from the common pool. If your estimate is correct, you will get an additional 100 Talers.

To elicit emex_amount:

Please estimate the average amount of Talers withdrawn from the common pool by all members of your group. If your estimate is correct with toleration of 13 Talers you will get an additional 100 Talers. Example: In fact, the average amount of Talers withdrawn from the common pool is 125. If you estimate any number between 112 and 138, your estimation will be correct and you will get an dditional 100 Talers.

To elicit noex:

Please answer the following question: Do you think it is appropriate to withdraw money from the common pool?

Please estimate how many members of your group including you answered positively to the previous question? If your estimate is correct, you will get an additional 100 Talers.

In "cold-response"-method treatment:

To elicit emex_members:

Please estimate how many members of your group including you have taken Talers from the common pool in each situation. If your estimate is correct within a toleration of 13 Talers you will get an additional 50 Talers for each correct answer. Example: In fact, the average amount of Talers withdrawn from the common pool is 125. If you estimate any number between 112 and 138, your estimation will be correct and you will get an additional 50 Talers.

Situation 1: "Twenty four participants in a previous session have made their decisions.

The six smallest withdrawals were: 0,0,0,0,0,0 Talers.

Situation 2: "Twenty four participants in a previous session have made their decisions.

The six biggest withdrawals were: 250,250,250,250,250,250 Talers.

To elicit emex_amount:

Please estimate the average amount of Talers withdrawn from the common pool by all six members of your group in each situation. You will get an additional 50 Talers for each correct estimate:

Situation 1: "Twenty four participants in two previous sessions have made their decisions.

The six smallest withdrawals were: 0,0,0,0,0,0 Talers.

Situation 2: "Twenty four participants in two previous sessions have made their decisions.

The six biggest withdrawals were: 250,250,250,250,250,250 Talers.

To elicit noex:

Please answer the following question: Do you think it is appropriate to withdraw money from the common pool?

Please estimate how many members of your group including you answered positively to the previous question? If your estimate is correct, you will get additional 100 Talers.

D Variable description and summary statistics

Variable Name	Variable Description	Control		PosInfo		NegInfo		Cold			
								ColdPos		ColdNeg	
		Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
w	Amount a participant withdrew from the Common Pool	140.75	112.15	88.27	104.95	156.75	91.04	122.36	114.20	150.97	111.7
Withdraw	Dummy variable, equals one if money is withdrawn and zero otherwise	0.67	0.48	0.56	0.50	0.83	0.38	0.61	0.49	0.69	0.47
PosInfo	Dummy variable, equals one if Treatment is PosInfo and zero otherwise										
NegInfo	Dummy variable, equals one if Treatment is PosInfo and zero otherwise										
ColdPos	Dummy variable, equals one if Treatment is Cold and a group if ColdPos and zero otherwise										
ColdNeg	Dummy variable, equals one if Treatment is Cold and a group if ColdNeg and zero otherwise										
Male	Dummy variable, equals one if a participant is male and zero otherwise	0.47	0.51	0.60	0.49	0.46	0.50	0.75	0.44	0.75	0.44
Age	Age of a participant	23.19	3.06	23.19	2.07	23.71	2.78	24.83	2.80	24.83	2.80
Econ student	Dummy variable, equals one if a participant is a student of either Economics or Economic Mathematics and zero otherwise	0.21	0.41	0.38	0.49	0.52	0.51	0.56	0.50	0.56	0.50
emex_amount_perc	Empirical expectations, belief of the average amount withdrawn, in percent	60.17	24.88	45.41	27.40	69.86	22.47	44.22	25.98	73.02	29.44
noex_perc	Normative expectations, in percent	55.56	30.62	53.13	35.34	65.97	30.55	63.43	35.15	63.43	35.15
Observations		48		48		48		36		36	
emex_members_perc	Empirical expectations, belief of the number of group members who withdrew some money, in percent	73.61	30.14	55.21	31.73	89.93	17.44	59.90	30.19	79.03	32.48
Observations		48		48		48		32		31	

Chapter 2

An Experimental Study on the Wage-Corruption Trade-Off[1]

Abstract

I question the rationale behind previous research results on the relationship between officials' wages and the level of corruption. In light of counterintuitive empirical findings by Schulze et al. (2016) and Chen and Liu (2018), I investigate this wage-corruption relationship by means of a laboratory experiment, using a robust version of a bribery game with four different relative wages for public officials. I also introduce a new method of assigning different wages to participants that helps to reduce self-selection based on ability, but grows out of a sense of entitlement to the endowment. The main findings are that (i) the effect of wages on the propensity of officials to accept bribes is negative, and (ii) the effect of wages on officials' propensity to act on them (i.e., reciprocate a bribe by providing a corrupt service) tends to be U-shaped. These findings may explain why the majority of experimental studies find this dependence to be negative and linear while Schulze et al. (2016) and Chen and Liu (2018), using hard data, find it to be U-shaped.

Keywords: corruption, wage, illegal behavior, experiment, bribery game
JEL classification: D73, J3, K42, C92

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2.1 Introduction

Corruption is viewed as one of the most severe and persistent obstacles to a country's economic and social development (Cieślik and Goczek (2018), Treisman (2000), Mauro (1995) and Rose-Ackerman (1978) among others). As a consequence, finding efficient ways to fight corruption has become a prominent topic for many social scientists. In 1974 Becker and Stigler, drawing on the efficiency wage literature, suggested the existence of a negative effect of high wages on the level of corruption. Since then, whether increasing officials' salaries can be an effective tool of anti-corruption policies (alongside a nonzero probability of being caught and punished) has been an intrinsic question for anti-corruption policymakers. The successful application of this tool in Singapore and the relative simplicity of its implementation has made it very tempting for policymakers elsewhere[2]. Unfortunately, implementation has been expensive and some empirical evidence (e.g., Schulze, Sjahrir, and Zakharov, 2016; and Chen and Liu, 2018) suggests that the relationship between officials' wages and the level of corruption is nonlinear and, in fact, U-shaped. Thus, before increasing officials' salaries in order to fight corruption in a given country, it is advisable to better understand the nature of the wage-corruption relationship.

In this paper, I address this relationship by means of an experiment. My study contributes to the existing literature, which has not reached a consensus on the nature of the wage-corruption relationship, and assumes that raising officials' wages is a good instrument of anti-corruption policy, but debates what amount would be optimal. While a considerable number of studies on corruption – arguably in line with intuition – find the wage-corruption relationship to be negative (i.e., An and Kweon, 2017; Lindkvist, 2014; Armantier and Boly, 2011; Van Veldhuizen, 2013; Besley and McLaren, 1993; Chand and Moene, 1999; and Di Tella and Schargrodsky, 2003), other studies contradict these findings, suggesting that an increase in the officials' salary may either be ineffective (Alt and Lassen, 2014; Abbink, 2000; Treisman, 2000) or, under some conditions, even encourage corruption (Sosa, 2004; Jacquemet, 2012; and Navot, Reingewertz, and Cohen, 2016). Finally, recent studies by Schulze et al.(2016) and Chen and Liu (2018) find the wage-corruption relationship to be U-shaped; the level of corruption decreases with an increase in the relative

[2]in countries like Peru, Argentina, Georgia, Nepal, Ghana (Schulze et al., 2016).

salary[3] at low and medium salary levels but seems to do so with diminishing returns, and it rises again after the relative wage exceeds a certain threshold.

One reason for the contradictory existing evidence on the link between corruption and wages is the difficulty of acquiring high-quality corruption data which, because of the nature of the issue, are not easily found in the real world or are difficult to interpret. Thus, the empirical evidence, based on observational data, as I will show in Section 2.2, is possibly marred by measurement error and other confounds that make the conclusions questionable.

These problems have motivated laboratory experimentation on corruption. In experiments, individuals' behaviour can be observed in controlled environments that allow the experimenter to see the effect of manipulation relatively free of confounding factors encountered in the field[4] (Dusek, Ortmann and Lizal, 2005; Frank, Lambsdorff, and Boehm, 2011; and Abbink, 2006, among others). The existing experimental studies on the wage-corruption relationship, however, also have limitations. For instance, they compare subjects' behaviour in only two treatments – low-wage and high-wage. If the wage-corruption relationship is indeed U-shaped, as Schulze et al. (2016) and Chen and Liu (2018) claim based on their data, using only two wage levels would not reveal the true pattern.

In this paper, I study experimentally the influence of wage increases on corruptibility in four treatments with different relative wages. Thus, by allowing for multiple wage levels my experiment allows me to study possible non-monotonicity in the wage-corruption relationship. The participants in my experiment, playing the role of public officials, decide whether to accept the bribes offered by the firms and then whether to act on them (provide a corrupt service). If the bribe is accepted and the corrupt service is provided, the sum of money is deducted from a good cause – a real-life charity that in my study represents social welfare. During the four treatments of the experiment, I manipulated the initial endowment of public officials in order to observe how their propensity to take a bribe and act on it (provide a corrupt service) would change.

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[3] Official's salary divided by the reference wage, which in this study is the wage of white-collar workers.
[4] Of course, experimentation itself has its fair share of challenges and confounds that need to be duly addressed; see, for example, Armantier and Boly, 2008; Abbink, 2006; Dusek, Ortmann, and Lizal, 2005. Finding the same result with field data and in the lab would give more confidence to the finding.

Unlike previous experimental studies, I also introduce an additional earnings stage where the participants earn their initial endowments by taking a test. The importance of earning the endowment has been shown in many studies (Oxoby and Spraggon, 2006; Cherry, Frykblom, and Shogren, 2002; Carlsson, He, and Martinsson, 2013; and Cherry and Shogren 2008; among others). By having earned an experimental endowment, participants feel entitled to implement outcomes that may be considered unfair otherwise (Oxoby and Spraggon, 2006). Specifically, participants may view random assignment as unfair and trigger attempts to reduce this perceived unfairness by making different choices than they would make in the case of a "fair" non-random assignment.

In the context of my experiment, being randomly assigned to the role of a firm, the initial endowment of which, in some treatments, can be much smaller than that of an official, may induce the firm to offer bigger bribes in order to possibly obtain a greater payoff and reduce the gap between the firm's income and that of the official. It may also affect the behaviour of the official by, for example, inducing the official to choose the corrupt option more often in order to reward the lower-paid firm and reduce the inequality. Thus, introducing an earnings stage helps to reduce or eliminate the influence of reciprocity or fairness considerations on participants' decisions. It also means that the participants value the experimental money more and thus make their decisions with greater caution. This condition makes the experimental design relate more closely to the real-life version of the corruption experience than the designs without the earnings stage.

The existing methods of earning roles and/or initial endowments, however, are subject to their own criticism as they use a performance-based approach. Thist may lead to a sample selection problem since the division into groups depends on individuals' observable and unobservable abilities, which may also determine the choices a participant makes during the experimental game. The originality of my approach lies in a combination of the random and non-random methods of assigning roles to participants: It takes the best of both approaches and reduces their drawbacks. Thus, the participants' roles are assigned based on the number of questions answered correctly in a test (performance-based assignment). However, instead of giving the same set of questions to all subjects, I randomly distribute sets with different degrees of difficulty (about which the subjects are not informed)[5]. Participants who get easy questions are expected to answer correctly, on

[5] In the instructions distributed to the participants I state that the participants get a set of 10 questions and have 10 minutes to answer them (see Appendix 2 B). I do not say that all participants get the same test. Thus, although I do

average, more of them than those who get difficult questions[6]. After the subjects finish the test, they are ranked based on the results of the test starting from '1' – having the worst performance to 'n' – having the best performance (where n is the number of participants)[7]. Those participants whose rank is lower than the median rank are awarded the role of a firm. The rest of the participants are awarded the role of an official.

Given that different sets of questions are distributed among the participants randomly, the number of correct answers does not directly depend on subjects' abilities, which helps to reduce selection into roles based on participants' personal characteristics[8] (in Section 2.4 I provide some evidence in favour of this claim). On the other hand, since the assignment into roles appears to be based on performance, the participants have a feeling that they are entitled to the money they earned and the roles awarded are deserved. This allows me to eliminate the possible effect of the inequality aversion on participants' decisions.

The results of my experiment suggest that corruptibility tends to be a negative function of officials' relative wage. Unlike the previous study of Van Veldhuizen (2013), which is without an earnings stage, the participants behaved more selfishly,i.e. participants accepted bribes more often even in the high-wage treatments (the significance of all the main coefficients of interest is less prominent and appears only in regressions with the full set of controls). The findings also show that the relationship between wage and propensity of officials to act on a bribe may be U-shaped, which may explain why the results of the majority of experimental studies on the wage-corruption relationship are different from those drawn by Schulze et al. (2016) and Chen and Liu (2018) who use hard data. When the bribe is not reciprocated and the corrupt service is not provided, the incidents of corruption may be less likely to be investigated and then registered as such by the police, as there are no visible consequences of such acts (no negative externalities) other than the

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not disclose all experimental conditions to the participants, neither do I deceive subjects by providing misinformation. According to the widespread agreement among researchers, this is not considered deception. For full discussions of what deception is refer to Hertwig and Ortmann (2008). .

[6] These expectations were confirmed in the pilot experiment.

[7] If two or more participants answer the same number of questions correctly the computer will randomly rank order them.

[8] More able participants are still expected to perform better than others regardless of the set of test they get. But these sets are designed in such a way that the probability of answering the questions from a difficult set is low for the students (which made up the majority of the subject pool) and that from the easy set is high. As a result, a participant's ability is no longer the main determinant of her performance in the test.

offence of a briber. By using incidents of corruption as a main proxy for corruption Schulze et al. (2016) and Chen and Liu (2018) probably based their research mostly on those corrupt acts that were reciprocated by officials. In my experiment, when I restrict my analysis to the corrupt acts that were reciprocated I also get a U-shaped wage-corruption relationship.

2.2 Literature review

The present paper challenges the findings of most studies on the wage-corruption trade-off, in which the prevailing view is that the relationship is negative[9]. The rationale for this relationship is as follows. First, an increase in public officials' wages makes corruption "more costly" to them. In particular, a long literature on efficiency wages (see Besley and McLaren, 1993, and Ades and Di Tella, 1999, among others) explains that when deciding whether to be involved in a corrupt deal, officials compare the benefits of corruption (i.e. bribe) to the losses they would incur if caught and punished. A higher salary increases their expected losses from corruption and, given a fixed probability of being caught and punished, makes it less tempting to be involved in a corrupt act (see Becker and Stigler, 1974; and Olken, 2007, among others). Second, an increase in wage might provide officials with more appreciation for their job, which also increases the losses from corruption by adding 'moral' costs to them (e.g., Van Rijckeghem and Weder, 2001; Abbink, 2000). Third, inequality-averse officials may tend to accept bribes in order to decrease the gap between their income and a higher comparison wage (the wage for a similar job type) or what they consider a 'fair wage' for their job. A rise in wage decreases this income gap and thus diminishes the incentive to accept the bribe[10]. And finally, higher salaries may attract more competent and more honest applicants (Abbink, 2000).

[9] A full review of the studies on the wage-corruption relationship can be found in Gans-Morse, Borges, Makarin, Mannah-Blankson, Nickow, and Zhang (2018).
[10] For a complete review see van Veldhuizen (2013).

A number of studies have provided evidence in favour of the negative wage-corruption relationship. Bond (2008), for example, shows that the high-corruption equilibrium in the theoretical model of court corruption can be eliminated when court officials are paid salaries sufficiently above the market-clearing rate. Goel and Rich (1989), in their empirical research using national-level (USA) cross-sectional data, find a significant negative effect of civil-service salaries on corruption. Using observational data on corruption, however, may lead to contamination of the results by measurement and endogeneity biases. The key reason is that the illegal nature of corruption makes it hidden from the public. Thus, researchers can only observe the cases of corruption reported by the police (Serra and Wantchekon, 2012; Banuri and Eckel, 2012). Another potential problem with such studies is how to choose an appropriate denominator, the reference wage, for the relative wage variable. Reference wage is important for understanding how big or fair one's wage is perceived to be. It also indicates the outside option of being a public official and, as Schulze et al. (2016) point out, is correlated with official's temptation to accept bribes. If the reference wage is not taken into account or is chosen incorrectly, the true relationship between wages and corruption cannot be found (Van Veldhuizen, 2013). Goel and Rich (1989), however, do not use any reference wage. These arguments cast doubt on the reliability of the findings of Goel and Rich (1989).

Armantier and Boly (2008) and Van Veldhuizen (2013) study the wage-corruption nexus using an experimental approach that tends to address the problems of measurement error and endogeneity. Armantier and Boly (2008) explore the effect of an increase in wage on teachers' corruptibility using both data collected in the field and in the experimental laboratory. Similarly to Goel and Rich (1989), they conclude that an increase in a teacher's wage decreases her corruptibility. This effect, however, holds only in the lab with a large set of controls but disappears in the field. Finally, Van Veldhuizen (2013), to which my paper closely relates, conducts a repeated fixed-pairs corruption experiment, with two possible payments given to officials and a charity to which the participants can donate, which absorbs the negative externalities of corruption. The author's main finding is that a pay raise significantly decreases the level of officials' corruptibility. In their experiment, however, the author compares only two different relative wages. However, if the relationship is non-linear, as suggested by Schulze et al. (2016) and Chen and Liu (2018), more relative wages should be investigated. Another possible limitation is that the roles of firms and officials as well as initial endowments were distributed randomly. Thus, the participants in the

experiment of Van Veldhuizen (2013) may have treated their endowments and payoffs with less caution or their behaviour may have been influenced by fairness considerations. Indeed, the results of my experiment support these conjectures, as the participants exhibited more selfish (accept bribes more often) behaviour (I discuss this in detail in Section 2.4.4).

Other authors find the effect of wage on corruptibility to be very small or find no effect at all. Van Rijckeghem and Weder (2001), while studying the salary-corruption trade-off using cross-country data, find that civil servants do not engage in less corruption when paid more unless the salary is very high. Moreover, the effect disappears completely in the within-country regression. This, however, signals that the between-country analysis may not control for some factors, thus biasing the results. Also, the study is widely criticized for using corruption perceptions as a proxy for corruption[11].

No effect was also found in a study by Abbink (2005), who investigated the wage-corruption dependence in a bribing game, varying the relative wage of public officials with respect to the wage of a third party. According to Van Veldhuizen (2013), however, they obtain such a result because there is no interaction between public officials and the third party, whose only role is to absorb the negative externalities. Frank and Schulze (2000) also do not find any significant effect in their one-shot game where they examine the change in the level of corruption with the change in the fixed payments to officials. This, however, may be because a one-shot game does not allow for the establishment of a long-term corruption relationship between a briber and an official. According to Van Veldhuizen (2013) and Frank et al. (2011) trust among partners is an essential component of corrupt transactions as their agreement cannot be enforced by the law. Thus, it is important to use a repeated game with fixed pairs of firms and officials to provide the opportunity for them to develop trust and establish a corrupt relationship.

The third group of studies show that under certain conditions a pay raise may even encourage corruption. Sosa (2004), for example, in a theoretical study suggests that higher salaries may reduce risk aversion which, if penalties are sufficiently low, leads to an increase in the level of corruption. Jacquemet (2012) finds that corruption increases with the wage in a three-player

[11]According to Schulze et al. (2016) and Donchev and Ujhelyi (2014) corruption perceptions reflect the real level of corruption in a country only very inaccurately.

corruption game with delegation, but the author's use of two different reference wages in the experiment may lead to inconsistent results.

Schulze et al. (2016) and Chen and Liu (2018) found the wage-corruption relationship to be U-shaped, using empirical data from the Russian Federation and China, respectively. Although their finding may explain why some studies find a negative effect of wage increase on the level of corruption, and others find no or even a positive effect, their result may be unreliable for several reasons. First, it seems to suffer from a measurement problem common to studies using observational data on corruption. Second, Schulze et al. (2016) cover officials occupying different positions with executive and legislative powers at federal, regional and local levels. The authors assume that these civil servants have similar types of jobs and thus are comparable. It is likely, however, that a local civil servant responsible for socio-economic planning of a small city has access to smaller public funds than a tax officer in Moscow, and thus tends to have fewer opportunities and temptation to be involved in a corrupt deal. Similarly, Chen and Liu (2018) cover all officials' positions and ignore their responsibilities which tend to be correlated with both officials' corrupt opportunities and their wages. Thus, comparing the propensities of such officials to act corruptly would be contaminated by endogeneity bias.

Another potential limitation of Schulze et al. (2016) and Chen and Liu (2018) concerns the variable the authors use as a proxy for corruption: the number of corruption incidents among officials (in Schulze et al.) and the amount of bribe offered taken from the data on court records on corruption convictions (in Chen and Liu). It is possible that the police, given their limited time and resources, choose to investigate better-paid officials more often than their lower-paid counterparts, thus increasing the probability of finding corruption incidents among the former. On top of these problems, the U-shaped wage-corruption relationship reported in Schulze et al. (2016) is based on very few data points in the range of relative salaries of 1.5 and higher. In sum, there are considerable questions about the explanatory power of the results obtained by the authors.

Finally, both Schulze et al. (2016) and Chen and Liu (2018) conclude that the wage-corruption relationship is U-shaped, based on the fact that the square of the relative-wage variable is positive and significant. Technically, this, however, only indicates that the relationship is convex and not necessarily U-shaped.

2.3 Experimental design and implementation

The objective of the experiment is to examine the effect of increased wages on the willingness to behave in a corrupt manner. There are four treatments (WT1-WT4). In each of these treatments I vary officials' relative wages in order to observe if, and how, their propensity to accept bribes and return corrupt service changes. In each treatment the experimental subjects participate in a bribery game.

2.3.1 The bribery game

My point of departure is a variant of Van Veldhuizen's (2013) two-player sequential game featuring firms and public officials. The roles of the participants in my experiment are assigned based on the results of the earnings stage described in detail in Section 2.1. Fig. 2.1 depicts the game stages and the payoff structure. At the beginning of each round participants are endowed with the amount of experimental money they earned during the earnings stage (initial endowment)[12]. In my experiment, the initial endowment of the firm serves as a base for the relative wage of the official. The firm moves first, deciding whether to make a transfer t (whether

[12] For public officials this money represents their wages. For the firms it represents their profits.

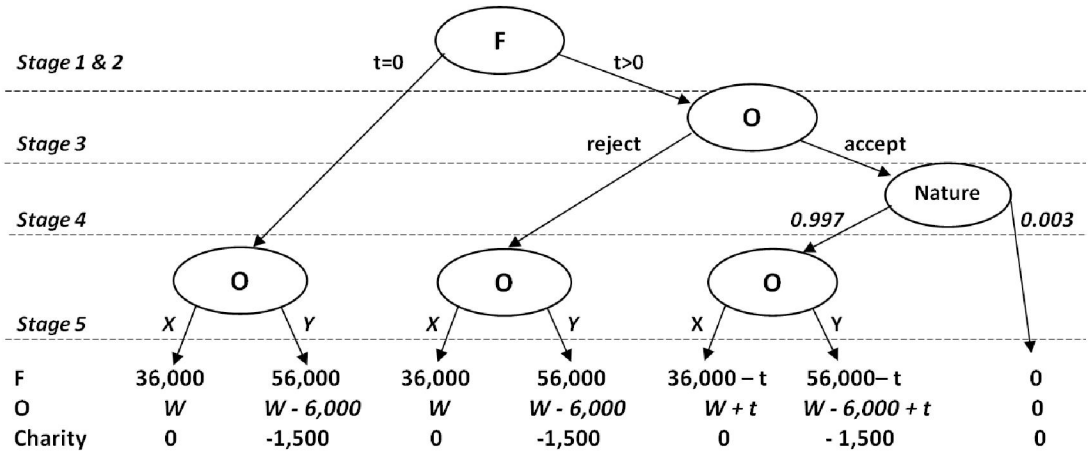

Figure 2.1: Experimental game: F – firm; O – official; t – a transfer/bribe; W – official's wage (varies with the treatment).

to give a bribe) to the official in order to entice the latter to choose the outcome favourable to the firm. This outcome benefits the firm at a cost to societal welfare, which in my experiment is represented by a charity. The firm also decides on the amount of the transfer, which can be any integer number between 1 and initial endowment (wage)[13]. If the firm decides to make a transfer to the official, then the official decides whether to accept it. When the transfer is accepted, there is a probability P, P = 0.003, that both parties will be caught and punished. The punishment in the experiment is loss of all earnings (except for the show-up fee) and disqualification from the experiment. The punishment, apart from following precedent, is set to be high in order to mimic the possibility of being fired from a job and/or being jailed if caught behaving corruptly in real life.

With the probability 0.997, however, both players stay in the game and the official decides which option to choose, X or Y. When he chooses the neutral option X, the official accepts t but does not provide a service to the firm (i.e., the firm does not benefit from corruption). When he chooses the corrupt option Y, the official takes the bribe and provides the service to the firm (i.e.,

[13] Unlike van Veldhuizen (2013), who does not limit the maximum amount of bribe offered by a firm, we set maximal *t* equal to the initial endowment in order to avoid participants ending up with negative payoffs. This is different from Abbink et al. (2002) who set maximal t equal to nine experimental units. In the experiment of Abbink et al. (2002) the negative externalities of corruption affect the participants themselves, so the maximal possible loss that a participant in the role of a firm can incur is bigger than the amount of bribe she offers. In my experiment, the negative externalities are imposed on a third party (a charity) and thus the maximal possible loss equals the offered bribe.

the firm obtains the benefits from corruption). Provision of a corrupt service is costly to the official since he has to apply effort to either provide a corrupt service or to justify his choice to colleagues and superiors or both. So the official strictly prefers option X to option Y. When the corrupt option Y is chosen a substantial sum of money is deducted from the third party, represented by a real-life charity. This last action mimics the negative externalities that corruption imposes on society[14]. It also gives a negative connotation to giving/accepting a transfer t in the game. The more corrupt the deals, the more money is deducted from the amount reserved for the charity and the less money is donated at the end of the experiment. Clearly, in the staged game the firm favours option Y. After all the decisions are made, the payoffs are realized.

The staged game parametrization is similar to that in Van Veldhuizen (2013)[15] (Fig. 2.1). Table 2.1 shows the wages for different treatments. It can be seen that the wage does not increase in equal increments. The reason is as follows. The values of the first two officials' wages W are identical to the relative wages of Van Veldhuizen (2013) and the last two are chosen in a way that makes the relative wage correspond to the high relative wages (2 and 2.5) of Schulze et al. (2016). Such a choice makes it easier to compare the results of my study to those of Van Veldhuizen (2013) and Schulze et al. (2016).

Table 2.1: Officials' wages over treatments

	Treatment 1	Treatment 2	Treatment 3	Treatment 4
Wage, W	36,000	56,000	72,000	90,000

[14]A real-life example of negative externalities of corruption: A construction firm uses materials that do not meet a minimum quality standard and bribes a public official to convince the latter to give her a tender for constructing a road. If an official gives her this tender, the citizens will suffer from the bad quality of the newly built road.
[15]Van Veldhuizen (2013) sets W equal to 36 and 56. We decided to scale up these numbers in order to make them corresponding to the real-life wages of public officials in the Czech Republic (according to the Informational System on Average Earnings (ISPV) an average monthly wage of a public official in 2015 was around 30,000 CZK).

2.3.2 Hypotheses

The experimental game described is meant to test the following four hypotheses.

Hypothesis 1a: An increase in officials' wages leads to a decrease in the probability of accepting a bribe.

Hypothesis 1b: The relationship between officials' wages and the probability of accepting a bribe is U-shaped: An increase in officials' wages leads to a decrease in the probability of accepting a bribe until the wage reaches a threshold after which the probability starts increasing again.

Hypothesis 2a: An increase in officials' wages leads to a decrease in the probability of acting on the bribe.

Hypothesis 2b: The relationship between officials' wages and the probability of acting on the bribe is U-shaped: An increase in officials' wages leads to a decrease in the probability of acting on the bribe until the wage reaches a threshold after which the probability starts increasing again.

It is worth noticing at this point that the subgame-perfect Nash equilibrium of the staged game is for the official to always choose option X and for the firm to always make no transfer. In a finitely repeated game, the equilibrium remains the same: In the last period the official will choose option X, disregarding the behaviour of the firm and therefore there is no reason for the firm to offer a bribe. Using backward induction it can be shown that this equilibrium holds for the remainder of the rounds of the finitely repeated game (Abbink et.al, 2002). Abbink et al. (2002) suggest, in other words, that the theoretical prediction is a non-corrupt society.

2.3.3 Experimental procedure

All experimental sessions were run at the Laboratory of Experimental Economics (LEE) at the University of Economics in Prague using zTree software (Fischbacher 2007)[16]. Subjects were recruited through an Online Recruitment System for Economic Experiments (Greiner 2004). In total, I used 160 subjects for the data collection (8 sessions of 20 subjects). The age of the participants ranged from 19 to 43 (with a median of 23) and the largest group (41%) mentioned Economics as their major.

After entering the laboratory, the participants were randomly assigned to their seats. The instructions for the experiments (see Appendix 1 B) consisted of two parts, which were distributed separately. The first part was given to the participants immediately and the second was distributed after everyone completed the earnings stage of the experiment. Both parts of the instructions were written using neutral terminology (transfer instead of bribe, Participant A and B instead of Firm and Official, etc.).

During the earnings stage the participants had ten minutes to complete the test, which consisted of ten questions. After completing the test they learned their roles (the roles stayed constant during all periods of the experimental game) and were given the second part of the instructions, which described the bribery game in detail. To make sure everyone understood the instructions the participants were asked several comprehension questions (see Appendix 2 C) and played two trial periods of the game before the beginning of the payoff-relevant periods. The printouts of the instructions remained with the participants and they could refer to them any time during the experiment.

After finishing reading instructions and answering comprehension questions the subjects were asked to choose a charity for the experiment from a list of five charities well-known in the Czech Republic: UNICEF, Red Cross, Greenpeace, the Catholic charity Caritas Czech Republic and People in Need (Člověk v Tísni)[17]. At the end of the session, the computer decided which

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[16] Following the usual practice, we randomized the order of the sessions to ensure that it does not influence the participants' choices. Thus, during the first day we ran three sessions in the following order: WT1, WT2, WT1. During the second day – WT3, WT3, WT4; and during the third day – WT2, WT4.

[17] These five charities are the most popular and well-known charities in the Czech Republic and cover a wide range of interests: promoting human rights (People in Need), social and health care (The Catholic charity Caritas),

charity out of the charities chosen by the participants would get the donation[18]. In total 900 CZK (approximately 35 Euros) was reserved for the charity at the beginning of the experiment.

Once the participants had chosen a charity, the experiment proper started. It consisted of twenty periods. At the beginning of each period the participants were given the initial endowment corresponding to their roles and the treatment. In every period the participants enacted the bribery game described above in fixed pairs. At the end of each period they learned their payoff for the period. At the end of twenty periods one round was randomly selected for cash payment[19]. The money deducted from the charity depended on the number of times option Y was chosen during the whole session. The winning charity was selected by the computer. Participants were then asked to fill in a questionnaire and were paid their endowments converted into the local currency (CZK) according to an exchange rate of 10,000 Talers (experimental units) to 45 CZK[20] in a separate room. The average cash payoff of participants was 333 CZK (13 Euros) (including the show-up fee of 100 CZK). The charity was given 587.75 CZK (out of 900 CZK or 22.5 Euros out of 35 Euros for a social welfare loss of 312.25 CZK or 12.5 Euros). The anonymous donations to the winning charities were made online after each treatment and the payment confirmations were sent out to the participants in the corresponding treatment.

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environmental issue (Greenpeace), protection of life and dignity of victims of armed conflicts (Red Cross) and children's rights (UNICEF). But even if a participant was reluctant to donate to any of the proposed charities, the fact of hurting a real-life charity should have added a negative connotation to the act of giving (for a Firm) or accepting and reciprocating (for an Official) a bribe.

[18] The computer randomly selected a participant whose choice was realized. Each participant had the same probability of being selected.

[19] We chose to pay for one randomly chosen period instead of paying cumulative earnings for the whole session in order to make decisions made in each round more thought through and independent from the (possible) outcomes of the (future) previous rounds. This method is used by, for example, Charness and Genicot (2009) and Fischer (2013).

[20]The exchange rate mimics that of van Veldhuizen but is adjusted to the difference in the level of prices between Amsterdam (where van Veldhuizen's experiment was conducted) and Prague (CPI equals 34%) and to the fact that the final payment was not the cumulative sum of earnings for twenty periods but a payoff from one randomly chosen period.

2.4 Results

In this section I discuss the results of the experiment. Section 2.4.1. demonstrates and analyses the results of bribe acceptance; Section 2.4.2 explores the propensity of choosing corrupt option Y versus the non-corrupt option X; Section 2.4.3 investigates the behavior of firms (in particular, how firm's choices influence the bribe acceptance rate among officials and their propensity to choose option Y); and in Section 2.4.4 I discuss how the results of the experiment were influenced by the introduction of the Earnings stage.

The analysis of the results of the Earnings stage shows that my manipulation worked: receiving a particular set of questions[21] determined the number of correct answers, and thus the participant's role in the experiment (see Fig. 2.2). This finding is confirmed by a regression of the number of correct answers on the set of questions and other determinants such as age, gender, maximum earned degree and major, where the set of questions is the only significant variable (p-value=0.004).

As for the main results, contrary to the theoretical prediction, positive transfers were made at least once in the vast majority of pairs (71 out of 80). The median number of periods in which a positive transfer was offered equaled 9 out of 20. The majority of pairs also chose option Y at least once in 57 pairs out of 80. The median number of periods option Y was chosen was equal to 4 (Fig. 2.3)[22].

[21] See Appendix 2 E for the description of how questions were selected for the Earnings stage.

[22] In order to compare my results with those of van Veldhuizen (2013) we made a separate analysis for the first two treatments. The median number of periods a positive transfer was offered during these treatments is equal to 9 out of 20 (compared to 8.5 out of 25 for van Veldhuizen). The median number of periods a Y choice was made is equal to 3 (compared to 3.5 for van Veldhuizen).

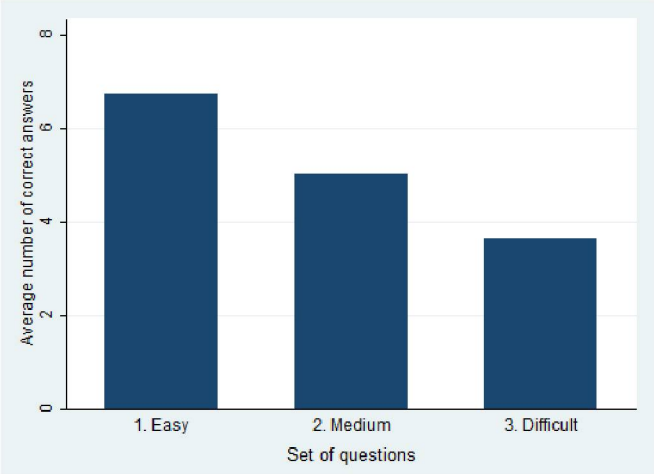

Figure 2.2: Average number of correct answers over different sets of questions during the earnings stage of the experiment[23]

Notes: The left box shows the fraction of periods when a positive transfer was offered for each official/firm pair. The right box shows the fraction of periods a Y choice was made for each official/firm pair. Every public official/firm pair is treated as one observation.

Figure 2.3: Incidence of transfers and Y choices.

Following Van Veldhuizen (2013) I exclude the official/firm pairs in which no bribe was ever offered since for studying official's corruptibility she has to be offered a bribe at least once

[23] Seven easy, seven medium and six difficult sets of questions were divided among twenty participants in a session. This proportion (7:7:6) remained the same in both sessions of each treatment.

during the experiment. In my experiment, a bribe was never offered in eight pairs: one in treatment WT1, one in treatment WT2, two in treatment WT3, and four in treatment WT4. Such an increase in the probability of never offering a bribe in higher treatments suggests that firms may anticipate higher-paid officials to be less corruptible (I explore this possibility in Section 2.4.3). After excluding these pairs, as well as two pairs that were disqualified and seven pairs in which either of the partners did not answer any of the comprehension questions correctly, 63 official/firm pairs[24] were left for further analysis. To test the hypotheses I used a between-subjects design.

2.4.1 Analysis of bribe acceptance

To analyze bribe acceptance, the level of corruption was measured by the number of transfers accepted by officials. This outcome variable was used to test Hypotheses 1a and 1b. The main source of identification is the variation in the initial endowment of officials.

Hypotheses 1a and 1b suggest that an increase in an official's wage leads to a decrease in the probability of accepting a transfer and this decrease is linear. To test these hypotheses I first compare the average number of bribes accepted in each treatment. On average, officials accepted 72% of offered transfers in treatment WT1, 65% in WT2, 71% in WT3 and 65% in WT4 (Fig. 2.4). Thus, the comparison of the means does not support Hypotheses 1a and 1b. Neither does the non-parametric Mann–Whitney estimation, which suggests that the difference between treatments is statistically insignificant (Table 2.2).

[24] It is noted that after the exclusion the current study may be underpowered and running additional experimental sessions may be needed in order to restore power (see Zhang and Ortmann (2013) for the explanation of the power of experimental studies).

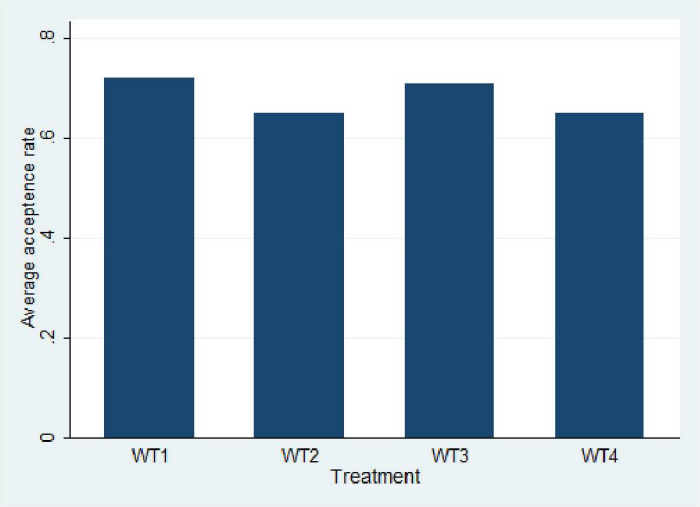

Figure 2.4: Average number of accepted offers over treatments conditional on transfer being offered

Table 2.2: Mann-Whitney test for the transfer acceptance rate[25]

Treatment	$N_{WT1/2/3}$	$N_{WT2/3/4}$	Z	p
WT1 and WT2	19	16	0.170	0.8649
WT1 and WT3	19	15	0.231	0.8176
WT1 and WT4	19	13	0.231	0.8176
WT2 and WT3	16	15	0.401	0.6887
WT3 and WT4	15	13	0.579	0.5626

Next, I analyze the results of the probit regressions of the decision to accept a transfer on treatment dummies (Table 2.3) using three specifications: (1) with no controls, (2) with the reduced set of controls[26], and (3) with the enlarged set of controls[27]. I also ran additional regressions (b) excluding the first five rounds of the game, as the participants might have still been learning the game during these first periods. The preferred specification was (3b), which

[25] Transfer acceptance rate was calculated as a ratio of the total number of accepted transfers by an official to the number of transfers offered to her.
[26] This is the set of controls offered by Van Veldhuizen (2013). It is included in order to compare the current results to the results of Van Veldhuizen.
[27] The questionnaire used to elicit the data for controls can be found in Appendix 2 D. The description of all variables and summary statistics is in Appendix 2 E.

Table 2.3: Probit estimates for bribe acceptance.

	Dependent variable: bribe accepted (1 = yes)					
	(1)		(2)		(3)	
	(a)	(b)	(a)	(b)	(a)	(b)
WT2	-.069	-.106*	-.110**	-.156**	-.069	-.111*
	(.051)	(.064)	(.052)	(.065)	(.051)	(.066)
WT3	-.012	-.087	-.063	-.146**	-.069	-.140**
	(.055)	(.064)	(.057)	(.069)	(.056)	(.068)
WT4	-.070	-.120*	-.151**	-.213**	-.125**	-.174**
	(.055)	(.063)	(.059)	(.073)	(.057)	(.071)
Male			-.069	-.069	-.075*	-.068
			(.047)	(.048)	(.045)	(.047)
Age			-.015**	-.016*	-.031***	-.031**
			(.006)	(.009)	(.008)	(.009)
Economics student			.021	.016	.049	.031
			(.040)	(.049)	(.039)	(.048)
Risk					.035***	.034***
					(.008)	(.009)
Highest degree					.1 56***	.136**
					(.042)	(.051)
Periods	All	5 to 20	All	5 to 20	All	5 to 20
Observations	609	434	609	434	609	434

Notes: This table displays the results of six probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only

*** Significant at 1%, ** Significant at 5%, *Significant at 10%.

includes an enlarged set of controls and accounts for the "learning period" during the first five rounds. I shall analyze this specification in detail. In this model, the main variables of interest -- treatment dummies - are all negative, significant and increasing (in absolute terms) in each treatment. Such results support Hypothesis 1a and can serve as evidence in favor of the possible negative relationship between wages and corruption. The additional controls -- risk and the highest degree earned -- are highly significant as well (for both models 3a and 3b) signaling the necessity of including them in the regression. Examining the rest of the controls, being older significantly reduces the probability of accepting the transfer while being a woman increases it, although this difference is insignificant. The latter finding is in line with Frank et al. (2011) who suggest that

women are more opportunistic than men when they do not expect negative reciprocity from their partners.

Table 2.4: Probit estimates for bribe acceptance: Check for convexity in the relationship.

Dependent variable: bribe accepted (1 = yes)		
	(a)	(b)
W	-1.1E-05	-3.2E-05
	(2.08E-05)	(2.75E-05)
W_2	3.34E-11	1.77E-10
	(1.63E-10)	(2.13E-10)
Male	- 0.236	- 0.226
	(0.134)	(0.151)
Age	-0.0922***	-0.0988**
	(0.023)	(0.0302)
Economics student	0.15	0.104
	(0.118)	(0.155)
Risk	0.109***	0.110***
	(0.0248)	(0.029)
Highest degree	0.473***	0.442**
	(0.129)	(0.168)
Periods	All	5 to 20
Observations	609	434

Notes: This table displays the results of two probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only
* Significant at 10%, ** Significant at 5%, *** Significant at 1%.

Economics students in my experiment tend to accept more transfers but this difference is also insignificant.

Table 2.3 also suggests that Hypothesis 1b about a U-shaped relationship between the acceptance rate and officials' wages should be rejected, as the marginal effects decrease with each treatment. In order to test if there is convexity in the relationship, however, I ran a polynomial Probit regression with Wage and Wage2. The model is tested with the enlarged set of controls (Table 2.4), with (a) and without (b) in the first five rounds, thus taking the learning period into account. Although the earlier coefficient of Wage2 (representing Wages2) is positive it is not significant, thus signaling the absence of a quadratic relationship between Accept and Wage. Therefore, Hypothesis 1b can be rejected.

When, however, I focus only on those cases when the accepted offer is reciprocated (option Y is chosen) there is a clear U-shaped relationship between the acceptance rate and official's wage (Table 2.5). The marginal effect decreases in WT2 and WT3 but then increases in

Table 2.5: Probit estimates for bribe acceptance if option Y was chosen.

Dependent variable: bribe accepted and option Y chosen (1 = yes)		
	(a)	(b)
WT2	-0.103**	-0.137**
	(0.051)	(0.066)
WT3	-0.227***	-0.337***
	(0.057)	(0.070)
WT4	-0.030	-0.048
	(0.058)	(0.077)
Male	-0.129***	-0.135***
	(0.044)	(0.052)
Age	-0.019**	-0.020*
	(0.007)	(0.010)
Economics student	0.042	0.052
	(0.041)	(0.052)
Risk	0.023***	0.017*
	(0.008)	(0.009)
Highest_degree	0.123***	0.144***
	(0.041)	(0.049)
Periods	All	5 to 20
Observations	609	434

Notes: This table displays the results of two probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only
* Significant at 10%
** Significant at 5%.
*** Significant at 1%.

WT4 and the difference between WT3 and WT4 is statistically significant[28]. This result, as I discussed in Section 2.1, supports the results of Schulze et al (2016) and Chen and Liu (2018) who find the wage-corruption relationship to be U-shaped using hard data. Interestingly, when only reciprocated offers are considered, I find that women are significantly less corrupt than men. This finding is in line with Frank, Lambsdorff et al. (2011), Frank and Schulze (2000), Swamy, Knack., Lee and Azfar (2001) and others who claim that women are less tolerant of corruption.

[28] This is suggested by the similar probit regression but with WT3 as a base dummy. The coefficient before WT4 in this regression is (0.197) and p-value is (0.001).

Finally, to conclude the analysis of officials' behavior I checked whether the decrease in their acceptance rate over treatments is connected to officials being more likely to reject smaller transfers during higher treatments. I found that officials do tend to reject smaller offers significantly more often in treatments WT2 - WT4[29]. Therefore, I can conclude that the differences in treatments can be linked at least partly to the monetary costs of corruption.

2.4.2 Analysis of the number of Y choices

The number of times option Y was chosen was used to test Hypotheses 2a and 2b. The main source of identification is the variation in the initial endowment of officials. Hypothesis 2a suggests that the propensity to choose option Y should decrease with initial endowment. A comparison of the average propensities to choose option Y among the four treatments, however, shows the U-shaped relationship. The probability of choosing option Y increases with the wage in treatments WT1-WT3 but then increases again in treatment WT4 when the relative official's wage is 2.5 (Fig. 2.5).

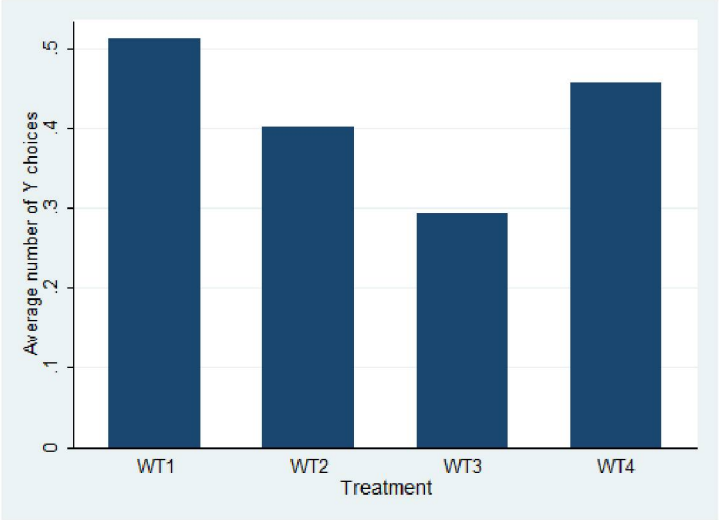

Figure.2.5: Average number of Y choices over treatments conditional on transfer being offered

[29] In a regression of the amount rejected on treatment dummies and personal characteristics the coefficients for treatments WT2-WT4 are 2266, 1900, 4028 and p-values are 0.000, 0.000, and 0.000, respectively.

Table 2.6: Mann–Whitney test for the share of Y choices[30]

Treatment	$N_{WT1/2/3}$	$N_{WT2/3/4}$	Z	P
WT1 and WT2	19	16	0.618	0.5364
WT1 and WT3	19	15	1.137	0.2557
WT1 and WT4	19	13	0.406	0.6848
WT2 and WT3	16	15	1.133	0.2571
WT3 and WT4	15	13	-1.689	0.0913

Table 2.7: Probit estimates for the number of Y choices.

Dependent variable: Option Y (1 = yes)						
	(1)		(2)		(3)	
	(a)	(b)	(a)	(b)	(a)	(b)
WT2	-.107**	-.160**	-.102*	-.147**	-.070	-.114*
	(.052)	(.062)	(.052)	(.065)	(.052)	(.066)
WT3	-.220***	-.372***	-.183**	-.330***	-.181**	-.320***
	(.058)	(.065)	(.060)	(.072)	(.059)	(.071)
WT4	-.053	-.083	-.021	-.049	-.001	-.022
	(.055)	(.068)	(.059)	(.078)	(.060)	(.078)
Male			.144**	.119**	.136**	.122**
			(.045)	(.054)	(.045)	(.053)
Age			-.006	-.003	-.017**	-.0174
			(.006)	(.010)	(.008)	(.011)
Economics student			.011	.036	.038	.052
			(.043)	(.054)	(.043)	(.053)
Risk					.026**	.022**
					(.008)	(.010)
Highest degree					.103**	.123**
					(.043)	(.049)
Periods	All	5 to 20	All	5 to 20	All	5 to 20
Observations	609	434	609	434	609	434

Notes: This table displays the results of six probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only.
* Significant at 10%
** Significant at 5%.
*** Significant at 1%.

[30] Share of Y choices was calculated as a ratio of the total number of Y choices made by the official to the total number the transfers offered.

The percentages of Y choices in treatments WT1-WT4 are 52%, 40%, 29%, and 46%, respectively. A non-parametric Mann–Whitney estimation, however, suggests that only the difference between WT3 and WT4 is weakly statistically significant (Table 2.6).

On the other hand, a probit regression of Option Y on treatment dummies and a constant shows that in treatments WT2 and WT3 option Y was chosen significantly less often than in a control WT1 treatment in five out of the six model specifications that I used. Similarly to the analysis of the acceptance rate I use three main specifications (Table 2.7): (1) with no controls, (2) with the reduced set of controls, and (3) with the enlarged set of controls. Each specification was run both for all data and for the last fifteen periods. As in the case of transfer acceptance, I chose model 3b: The two additional controls - risk and highest degree -- are significant, and thus should be included in the regression.

I shall analyze model (3b) in detail now. Two of the three main variables of interest --WT2 and WT3 dummies -- are negative and significant, which suggest that the increase in the initial endowment decreases the propensity of choosing corrupt option Y (in line with Hypothesis 2a) for relative wages equal to 1.5 and 2. The marginal effect of the WT4 dummy, however, shows that this propensity increases again in treatment WT4 when the relative wage is 2.5, and a Mann–Whitney estimation implies that this increase is significant (Table 2.6). This finding suggests a possibility of a U-shaped relationship between the propensity to choose corrupt option Y and officials' wages. Therefore, Hypothesis 2b cannot be rejected. In general the results obtained for the rest of the control variables fit well with the previous findings. Men tend to choose a corrupt option Y significantly more often than women. According to Frank et al. (2011) this may be because women, while being more opportunistic (which I saw in the results of bribe acceptance), are less inclined to cooperate when their partner is a briber. Age is found to be insignificant, although being less risk averse and having earned a higher academic degree are positive and significant. Similar to the analysis of bribe acceptance, the behavior of economics students did not significantly differ from that of non-economics students.

2.4.3 Firm's behavior

As the design of my experiment implies that the firm moves first, the decision made by the firm, such as how often to offer a bribe and of what size, can influence an official's response. In this subsection, I provide a short analysis of firm's behavior in an attempt to better understand what makes a corrupt deal successful. First, I analyze firms that never offered a transfer. As I mentioned before, the number of such firms increases from one in treatments WT1 and WT2, to two in WT3, and four in treatment WT4. Thus, it seems that Firms anticipate better-paid officials to be less corrupt. Indeed, the probit regression of the decision to never offer a transfer on treatment dummies and controls shows that the coefficients for treatments WT3 and WT4 are positive and significant and that marginal effects increase[31]. Expectedly[32], this finding appeared to be clearer for risk-averse individuals and economics students as their propensity to never offer a bribe was higher than that of their counterparts'. Men and older participants are also more likely to never offer a transfer, unlike the participants with a higher academic degree, but the latter is insignificant.

Second, I analyze firms who made a transfer at least once. It seems like those Firms who do decide to bribe better-paid officials try to make them act on a bribe by giving them and more frequent bribes. This effect is confirmed by the results of two regressions: a robust regression of the size of the transfer on treatment dummies and controls and a probit regression of the decision to make a transfer on treatment dummies and controls. While all the coefficients for treatment dummies are positive, they are significant only for treatments WT2 and WT4 and insignificant for WT3 in both regressions. Thus, neither of these two factors can explain the decrease in the acceptance rate of officials in treatments WT2, WT3 and WT4. They also cannot explain the increase in the propensity to act on a bribe in WT4, as the difference in the size and frequency of bribes is insignificant between the treatments. Thus, it is likely that the behavior of officials in the current experiment was at least partly driven by non-monetary considerations.

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[31] In the probit regression of the decision to never offer a transfer on WT2, WT3, WT4, for being male, age, an economics student, riskiness and highest degree, the coefficients are -.001, .057, .145, .034, .010, .158, -.020, .-003 and p-values are .977, .026, .000, .031, .004, .000, .000, .846, respectively.

[32] We expected economics students to be able to understand and follow the prediction of Nash equilibrium, which in my game is to never make a transfer.

2.4.4 The effect of the introduced earnings stage

In the first section I suggested that the earned endowments make participants more selfish than when the endowments are a windfall, which is in line with the findings of Cherry, Frykblom, and Shogren (2002), Carlsson, He, and Martinsson (2013) and Oxoby and Spraggon (2006). This effect can be seen when I compare the results of the current experiment with those of the similar experiment by Van Veldhuizen (2013), which is based on the same bribery game, with a positive probability of being caught and punished but with windfall endowments. Thus, in my experiment, a bigger increase in wage, i.e. double, is required to make people accept significantly fewer transfers than in Van Veldhuizen's experiment, where a 1.5 times increase in wage was enough for this purpose. I observe the same pattern in the analysis of the number of Y choices made by officials. The marginal effects in my experiment also differ from those of Van Veldhuizen (2013) being five times smaller in the propensity to accept bribes and three times smaller in the propensity to reciprocate it.

Having to earn the initial endowment also makes women accept corrupt transfers more often than in the experiments of Van Veldhuizen (2016), Frank, Lambsdorff et al. (2011), etc. Imposing negative externalities on a charity, however, seems to be an important determinant for women's decisions between corrupt and non-corrupt options, since they still choose option Y significantly less often than men. Finally, I did not observe any significant difference between the behavior of economics and non-economics students. This may suggest that the feeling of entitlement to the initial endowment in my experiment reduces the difference in the money-related decisions between the participants with different majors.

2.5 Conclusions

In this paper I study the relationship between officials' wages and their corruptibility using an experimental approach. Unlike previous studies I attempt to use a more robust design by introducing the earnings stage to the experiment, where the participants earn their initial endowment by taking a test. As a result, the participants' behavior in the experiment changes and becomes more opportunistic compared to similar experiments without an earnings stage. A similar result is obtained for the number of corrupt Y choices, which impose negative externalities on the charity chosen by the participants.

By allowing for multiple levels of wages I found that if I restrict my analysis only to reciprocated bribes, the relationship between the acceptance rate and officials' wages may be U-shaped; that is the acceptance rate falls until the wage reaches 2.5 and then starts increasing again. This result can probably explain the U-shaped wage-corruption relationship found by Schulze et al (2016) and Chen and Liu (2018). As in real life, it is difficult to observe cases of corruption not reciprocated by officials. The proportion of such "unfulfilled corrupt acts" in the hard data of Schulze et al (2016) and Chen and Liu (2018) may have been smaller than the proportion of the cases when the bribe was reciprocated. Thus, the authors saw a U-shaped wage-corruption dependence.

As expected, I also find the decrease in the propensity to act on a bribe to be U-shaped. The results show that when the relative wage of officials reaches 2.5, participants start behaving more unethically as the propensity to return a corrupt service, and thus impose negative externality on a charity, increases to almost the initial number as when the relative wage is1.0. For future research, it would be interesting to study possible reasons for such an increase.

Although this experiment was conducted with a relatively small number of participants who were mainly students with little or no experience in the public sector, it suggests a strong link between wages and corruption. My results likely represent the lower bound of the real-life effect, however. Also, using wages as a tool of anti-corruption policy should be applied with caution, because if the wage is set "too high", corruption can increase. Thus, it should be used in combination with other tools such as, for example, a high probability of being caught and punished for corruption. I expect that increasing the probability of detection could offset the reasons behind

the increase in the propensity to act on bribes when the relative wage becomes "too high," but further research is needed to confirm this hypothesis.

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Appendix 2

A Sample questions for the Earnings stage

Easy questions:

Of the following, which is greater than 1/2 ?

A: 2/5; B:4/7; C:4/9; D:5/11; E: 6/13

What is the capital of Austria?

A: Tirana; B: Andorra la Vella; C: Yerevan; D: Vienna

True or false? Pi can be written as a fraction.

A: Pi is not a number; B: It depends; C: False; D: True

Difficult questions:

Gordon is twice as old as Tony was when Gordon was as old as Tony is now. The combined age of Gordon and Tony is 112 years. How old is Gordon now?

Turbid is most similar in meaning to:

A: Shiny; B: Murky; C: Pellucid; D: Petrified; E: Agitated

In the two numerical sequences below, one number that appears in the top sequence should appear in the bottom sequence and vice versa. Which two numbers should be changed around?

100, 89, 76, 63, 44, 25

105, 93, 79, 61, 45, 25

A: 61 and 63; B: 44 and 45; C: 89 and 93; D: 100 and 105; E: 100 and 93; F: 89 and 79

B Sample instruction for treatment WT2

Instructions

Welcome to the experiment!

Please turn off your cell phone and other electronic devices now and leave them turned off for the remainder of the experiment.

General information on the experiment

You are going to participate in an experiment on decision making.

If you read the following instructions carefully, you will be able to earn an appreciable amount of money. How much you will earn depends on your and others' decisions. It is therefore important that you understand the following instructions.

The instructions consist of two parts. Part 2 of the instructions will be given to you after you complete Part 1. Part 1 of the instructions immediately follows these introductory explanations.

Once you finish reading Part 1 of the instructions press the button "Continue" on the computer screen.

- **Anonymity**

In the experiment, all interactions among participants will take place through the computer network. All participants decide anonymously, i.e., the other participants will not be able to find out the decisions you make during the experiment. To ensure anonymity it is imperative that all participants observe the following rule: **During the experiment all communication is prohibited**, i.e., you are not allowed to speak or otherwise express yourself. **Usage of any electronic device and the Internet is prohibited. If you violate these rules, you will be dismissed from the experiment and not be paid.**

- **Payments**

The currency used in this experiment is Talers. The total amount of Talers that you have earned during the experiment will be converted into CZK at the end of the experiment at an exchange rate of

10,000Taler = 45 CZK.

You will also receive a show-up fee of 100 CZK. You will be paid your earnings in cash, and privately, upon leaving the room.

To recall, today's experiment consists of two parts. During **Part 1**, or the Earnings Stage, you will be asked to take a test. Based on your performance during Part1, you will be assigned one of two types: **Participant A or Participant B.** The type will remain unchanged throughout the experiment.

During **Part 2**, you will be randomly paired with another person of the other type and will be asked to accomplish a task that you will be given for each of 20 periods. Each period you will be given an initial endowment to accomplish the task. **Participants A will get the endowment of 36,000 Talers and Participants B will get the endowment of 56,000 Talers.**

When you have finished Part 2 of the experiment, you will be asked to fill in a questionnaire. The answers you provide in this questionnaire are anonymous. Furthermore, your

answers to this questionnaire will not affect your earnings during the experiment but I will appreciate you answering carefully. While you fill out questionnaire I will prepare your earnings.

In the following pages the experiment is described in detail.

Instructions Part 1

During this stage you will be able to earn an appreciable amount of experimental money by answering questions from a test. The test will consist of 10 questions and all participants will have 10 minutes to answer them.

After you finish this test, all participants will be ranked based on the results of the test. If two or more participants answer the same number of questions correctly the computer will randomly determine the rank order among them. Half of the participants with the lower ranks will be given the role of Participant A. Other participants will be given the role of Participant B. The role that will be awarded to you will remain unchanged throughout the experiment.

If you finish reading press the button "Continue" on the computer screen.

Instructions Part 2

This is a decision-making part. The payment you receive at the end of the experiment depends on the decisions you make. Moreover, you will be able to earn money for a charity. I will explain this in more detail below.

This part consists of 20 periods. At the beginning of the first period, you will be randomly matched to a player of the other type. The player you are matched with will remain unchanged throughout the experiment.

Once you have read the information in this Part carefully you will be asked to do several computerized control exercises, which are designed to check that you have understood the decision situation. Apart from that you will be able to practice during two trial periods before the payoff-relevant periods begin.

Before you proceed, make yourself familiar with Figure 1 **which is supplied on a separate sheet**. It is useful to **relate every explanation that follows below to Figure 1.**

Decision Situation

Every period in this experiment consists of five stages, which will always take place in the following order:

Stage 1: Transfer or no transfer

Participant A decides whether he wants to transfer an amount *t*, $t > 0$, to Participant B. If he does, then the period is continued with Stage 2. If Participant A decides not to transfer a positive amount, i.e., $t = 0$, then the period continues with Stage 5.

Stage 2: The amount to be transferred

Participant A decides on the amount to be transferred to Participant B, *t*. The transferred amount *t* can be any whole number greater than zero and 36,000Talers ($0 \leq t \leq 36{,}000$).The period then continues with stage 3.

Stage 3: Acceptance or rejection of the transfer

Participant B then decides whether to accept the proposed transfer, *t*. If Participant B decides to accept it, the proposed amount is removed from Participant A's credit and added to Participant B's credit. The period then continues with Stage4. If Participant B rejects the transfer, then the credits remain unchanged. The period is then continued with Stage 5.

Stage 4: Possibility of Getting Disqualified

If Participant B decided to accept the transfer in Stage 2, a number ranging from 1 to 1000 is randomly drawn. If the number is 1, 2 or 3, then both Participant A and Participant B are disqualified (the probability of being disqualified is 0.003). That means that the experiment ends for these two players and all their previous earnings are canceled. (At the end of the experiment, both players receive only their show-up fee.) The two disqualified participants fill in a questionnaire when the experiment has ended. For the other participants, the experiment continues normally. If the randomly drawn number is 4, 5, ..., 999, or 1000 (which happens with probability 0.997), the period is continued with stage 5.

Stage 5: Participant B Chooses Between X and Y

Participant B chooses one of the alternatives *X* or *Y*. If Participant B selects alternative *X*, then his credit is increased by t and becomes (56,000 +*t*). The credit of Participant A is decreased by *t* and becomes (36,000 –*t*). The credit of the charity remains unchanged.

If Participant B selects alternative *Y*, then his credit is decreased by 6,000 Talers but is increased by *t* and becomes (50,000 +*t*). The credit of Participant A is increased by 20,000 and decreased by t and becomes (56,000 – *t*). The credit of the charity is decreased by 1,500 Talers.

There will be one charity that can benefit from this experiment. The charity starts off with a total of 200,000 Talers, which equals 900 CZK. The final donation depends on the decisions made by the participants in the experiment. The donation will be strictly anonymous; no mention will be made of either LEE or any participant of this experiment. The donation will be made online after the end of the experiment. The receipt for the donation will be sent to your email soon after the end of the experiment or can be found here:
https://www.dropbox.com/sh/0lx65mtcdukl7nx/AAAnWF22d-bO1879teoBoZX4a?dl=0
After Stage 5, the period has ended. Overall earnings are the sum of all changes of credits during the five stages of the period. The decision situation will be repeated for 20 periods.

- **Final earnings**

The earnings from **only one of 20 periods** will be converted into CZK and paid to you at the end of the experiment. The computer will randomly decide the period that will be paid out. As for the charity, the final donation will be equal (200,000Talers – n*1,500), where n is the number of times option Y is chosen **during 20 periods**.

- **Charities**

For this experiment, I have selected a total of five charities. You will be asked to select a charity at the beginning of Stage 2. At the end of the experiment, I will pick the charity selected by one randomly determined person.

A. UNICEF: Created by the United Nations. Its activities include promoting children's rights, and securing worldwide visibility for children threatened by poverty, disasters, armed conflict, abuse and exploitation.

B. Red Cross: Its official mission is ''to stand for the protection of the life and dignity of victims of international and internal armed conflicts.'' Amongst its activities, it attempts to organize nursing and care for those who are wounded on the battlefield; it also supervises the treatment of prisoners of war.

C. Greenpeace: is a non-governmental environmental organization. Greenpeace states its goal is to "ensure the ability of the Earth to nurture life in all its diversity" and focuses its campaigning on worldwide issues such as climate
change, deforestation, overfishing, commercial whaling, genetic engineering, and anti-nuclear issues.

D. The Catholic charity Caritas Czech Republic: is the largest charitable provider in the country for social and health care, providing accommodation, health services, integration projects, and general help for the aged, disabled, migrants, prisoners, women with children, and the poor.

E. People in Need (Člověk v Tísni): is a humanitarian organization. It concentrates on human rights, alleviation of poverty and the reduction of national prejudices and xenophobia.

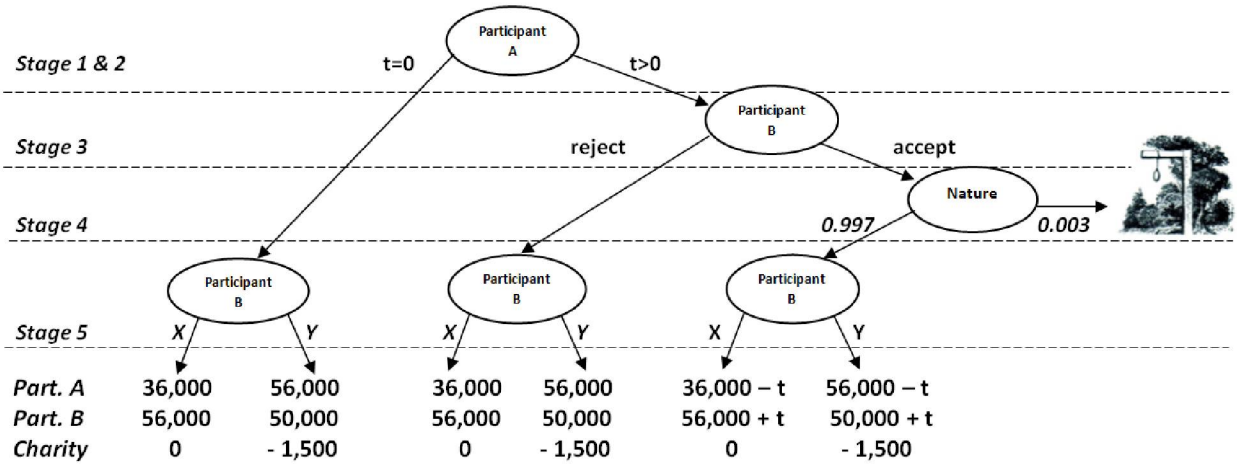


Figure 1: The experimental game tree

Notes: In the figure, the transfer is represented by t and X and Y are the two options that can be chosen by Participant B.

C Comprehension questions

[Comprehension questions were shown on the screen of the computers before the beginning of Part 2 of the experiment. Once a participant typed the answer he saw the correct answer and explanation.]

Question 1: Suppose Participant A has proposed a transfer of 8,000 Talers to Participant B. If Participant B accepts and the pair is not disqualified, what will be Participant B's earnings in Talers if option X is chosen?

Answer: Participant B's earnings in Talers will be: 56,000 + 8,000 = 64, 000

Question 2: What will be Participant A's earnings in Talers in this case?

Answer: Participant A's earnings in Talers will be: 36,000 - 8,000 = 28, 000

Question 3: What will be Participant B's earnings in Talers if option Y is chosen?
What will be Participant A's earnings in Talers if option Y is chosen?

Answer: Participant B's earnings in Talers will be: 50,000 + 8,000 = 58, 000

Participant A's earnings in Talers will be: 56,000 - 8,000 = 48, 000

Question 4: In this experiment, there are a total of 20 participants, such that there are 10 pairs. Suppose that in the first period there are 5 pairs in which Participant B chooses option Y and in the second period -- 6 pairs. How many Talers will the charity lose in total after the second period?

Answer: the charity will lose the cumulative amount for the two periods: 7,500 + 9,000 = 16,500 Talers

D Questionnaire (shown on the screen of the computers at the end of the experiment)

Thank you for participating in today's experiment. Please answer the following questions while the experimenter is preparing your earnings.

1.Please select your gender. Male / Female

2.What is your Major? _____

3.What is your year of birth? _____

4.What is your current study level?
A. High school;

B. Bachelor or equivalent;

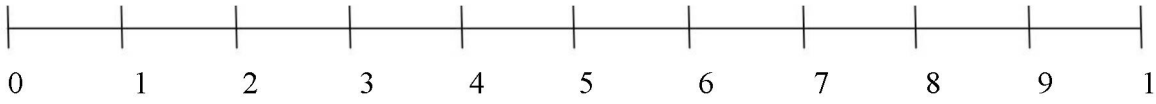
C. Master or equivalent;

D. PhD or equivalent;

5. How do you see yourself?

Are you generally a person who is fully prepared to take risk or try to avoid taking risk?

Please tick a number in the following scale, where the value 0 means not at all willing to take risks, and the value 10 means very willing to take risks‖.



0 1 2 3 4 5 6 7 8 9 10

Not at all willing to take risks

Very willing to take risks

E Selecting questions for the Earnings stage

In order to select the questions of different levels of complexity for the Earnings stage of the experiment, I conducted an additional session. For this session, I preselected 64 different questions: 32 easy questions and 32 difficult questions (sample questions can be found in Appendix 2 A). In total sixteen people participated in this session. The participants were divided into two groups of eight and each group obtained a different set of 32 questions (the mix of 16 easy and 16 difficult questions). Then I created a group of difficult/easy questions from the questions that nobody/everybody (8 participants) or almost nobody/everybody (7 participants) answered. From these questions I formed three sets of questions:

Table E.1: Distribution of easy and difficult questions between different sets of questions

Set of questions	Number of easy questions	Number of difficult questions
Easy	8	2
Medium	5	5
Difficult	2	8

With such a choice of proportion of easy/difficult questions I tried to avoid easy sets from being too easy (which may have lessened the feeling of entitlement to the earnings) and difficult sets from being too difficult (which may have caused frustration). During several pilot sessions I made sure such selection worked as I intended.

F Variable description and summary statistics

Variable Name	Variable Description	Descriptive Statistics							
		T1		T2		T3		T4	
		Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
WT2	Dummy variable for treatment 2								
WT3	Dummy variable for treatment 3								
WT4	Dummy variable for treatment 4								
Male	Dummy variable. Equals one if gender is male	.737	.441	.688	.464	.467	.500	.462	.500
Age	Participant's age	23.895	5.063	23.563	4.982	23.467	2.709	22.308	1.591
Economics student	Dummy variable. Equals one if the participant is/was an economics student	.474	.500	.375	.485	.333	.472	.615	.487
Risk	A participant's self-assessment of to what extent she is prepared to take risk: 0 - not at all willing to take risks, and the value 10 - very willing to take risks‖	5.895	2.867	5.063	2.049	5.733	2.466	5.462	2.065
Highest degree	The highest academic degree achieved by the participant at the moment of the experiment): 1=High school; 2= Bachelor or equivalent; 3=Master or equivalent; 4=PhD or equivalent.	1.421	.675	1.438	.705	1.533	.619	1.462	.636

Note: The summary statistics is provided for public officials only

Chapter 3

How Much Is Too Much? Self-Perceived Socio-Economic Status as an Explanation for the Positive Wage-Corruption Relationship: An Experiment [1]

I propose an explanation of the positive, and U-shaped, wage-corruption relationship found in previous research: When officials' salaries rise beyond a certain threshold, their self-perceived socio-economic status (SSES) increases and they become less averse to corrupt behavior. I investigate this theory using a laboratory experiment. In the experiment, public officials can accept a bribe and then decide whether to act on it. The act benefits the briber but imposes externalities on social welfare. I manipulate experimentally officials' SSES to test if there is some such effect and when it kicks in. The results reported in this paper suggest that the effect of SSES on the propensity of officials with the highest wage to accept and reciprocate bribes may be positive and significant if a bribe is high enough and insignificant overall. This result may challenge efficiency-wage based explanations that suggest that the higher a wage-premium, the less public officials will be tempted to accept and reciprocate bribes.

Keywords: corruption, wage, illegal behavior, social status, experiment, bribery game
JEL classification: D73, J3, K42, C92

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3.1 Introduction

It is widely accepted that the environment significantly influences individuals' behavior and decision-making processes (e.g., Smith, 1759; Smith, 1976, 1982). More recently, particular attention has been paid to social influences on choices to behave anti-socially (e.g., Chang and Laiy, 2004, Bicchieri and Xiao, 2009; Innes and Mitra, 2013). I study one of the aspects of this influence – the effect of self-perceived socio-economic status on one's corruptibility.

Self-perceived (or subjective) socio-economic status (SSES) is defined as the rank an individual perceives she has in society relative to others in terms of wealth, occupational prestige, and education (Piff, Stancato, Cote, Mendoza-Denton, and Keltner, 2012)[2]. SSES affects different aspects of human behavior such as, for example, choice of products (Stephens, Markus, and Townsend, 2007) and parenting methods (Lareau, 2003). It also seems that SSES can induce different types of unethical or anti-social behavior (Piff, Stancato, Cote, Mendoza-Denton, and Keltner, 2012; Van Doesum, Tybur and Van Lange, 2017; Piff and Robinson, 2017).

I investigate the dependence between an individual's SSES and her corruptibility using a laboratory experiment. Contrary to standard efficiency-wage explanations (which hold that the higher a wage-premium, the less public officials will be tempted to accept and reciprocate bribes), my experimental results provide evidence of why sometimes corruptibility may increase with wage: When the relative wage becomes sufficiently high, the SSES of the official switches from low or medium to high[3]. This switch, according to Kraus, Piff, Mendoza-Denton, Rheinschmidt and Keltner (2012), Van Doesum, Tybur and Van Lange (2017) and Piff and Robinson (2017) can give rise to a self-focused orientation which facilitates unethical behavior such as corruption. In

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[2]Unlike socio-economic status (SES), which is objective and is determined by wealth, education, and occupational prestige, SSES is subjective and is influenced by personal beliefs about one's own SES. SSES has been shown to be an important determinant of one's behaviour. Johnson, Richeson, and Finkel (2011), for example, show that middle-class students who come from rather wealthy families feel threatened when surrounded by a majority of even wealthier students at an elite university, and underestimate their SES (Kraus and Piff, 2012).

[3] This assumption is based on Miller's suggestion that income is one of the most important components of socio-economic status (SES) (Dabbs and Morris, 1990). While highly correlated with income (Dreger, López-Bazo, Ramos, Vicente Royuela, and Suriñach, 2015), wage is not the only determinant of SSES. It seems safe to say though that by studying the effect of changes in wage on SSES, we identify a key driver of the effect of SSES on corruptibility.

other words, when people become rich enough, they perceive themselves as "superior individuals" who feel entitled not to comply with the law as other people do[4]. Piff et al. (2012) provide considerable evidence of such mechanisms. My explanation is in line with the reference-dependent utility theory explained in Foltz and Opoku-Agyemang (2015). It suggests that when an official's wage becomes high enough their reference income level (which is the desired, or perceived-as-fair, level of income) increases, which in turn increases officials' corrupt behavior. The authors, however, do not provide empirical evidence supporting this theory.

Drawing on the results I obtained in Chapter 2 of this dissertation (henceforth Momotenko, 2019), I test my SSES theory using a laboratory experiment: Student subjects participate in a bribery game: Those in the role of public officials decide whether to accept a bribe offered by a firm and then whether to provide a corrupt service. Once the bribe is accepted, and the corrupt service is provided, a sum of money is deducted from a third party – a real-life charity, which in the context of this experiment represents social welfare. In the experiment, I manipulate the SSES of public officials to observe how their propensity to accept bribes and provide corrupt service changes. There are two sources of influencing participants' SSES: The first source comes from participants having to "earn" different initial endowments during the earnings stage. Those who "earned" the most (the least) are expected to get a feeling of superiority (inferiority) over the other participants. This feeling is expected to be enhanced by the second source of influence – manipulation of SSES using a method adapted from Piff et al. (2012), which itself is based on the MacArthur Scale of subjective SES (Adler, Epel, Castellazzo and Ickovics, 2000). In this latter method, the participants are asked a battery of questions where they have to imagine and describe their lifestyle and attitude to others in a hypothetical society where they would be as rich (poor) as they are in the present experiment. (The method of the experiment is described in detail in Section 3.3.1).

The results of my experiment contribute to the substantial literature on the wage-corruption relationship. This literature can be divided into three camps. In the first, the wage-corruption

[4] An illustrative case might be that of the (former and now again) president of Germany's most successful, and very rich, soccer club, Bayern Muenchen, Uli Hoeness.. Mr. Hoeness, who before his conviction for tax evasion was on excellent terms with Chancellor Angel Merkel, was sentenced by a German court to three years and six months in jail for having not paid around 27m Euros of taxes due. While commenting on the situation, German newspaper Stern.de said that Hoeness was a true "representative of the German elite" who thought that he was "above the law" (Güßgen, 2014).

relationship is believed to be negative. For example, the literature on efficiency-wage theory claims that an increase in an official's wage makes accepting bribes "more costly" in the case of being caught and punished and thus less likely(see Besley and McLaren, 1993; and Ades and Di Tella, 1999, among others). Another theory, that a positive correlation exists between wage and job appreciation (e.g., Van Rijckeghem and Weder, 2001; Abbink, 2000), suggests that a wage increase increases officials' job appreciation, which adds to the "costs of corruption" in the case of being caught and punished, thus decreasing officials' willingness to become corrupt. The second camp provides some evidence supporting the theory of a positive wage-corruption trade-off (e.g. Navot, Reingewertz, and Cohen, 2016; Foltz and Opoku-Agyemang, 2015; Gatti, Paternostro, and Rigolini, 2003; Sosa, 2004; Jacquemet, 2012; and, partially[5], Schulze, Sjahrir and Zakharov, 2016). Only a few studies in this camp, however, attempt to explain the intuition behind such a relationship. Sosa (2004) suggests that if penalties are sufficiently low, wage increases may lead to reduced risk aversion and, consequently, to increases in the level of corruption. Besley and McLaren (1993) show that for those 'superauditors' (who detect and prosecute corruption) who are corruptible themselves, the temptation to demand bribes from better paid corrupt officials is higher, as they can extract more money from those officials for concealing corrupt incidences. In this case the increase in an official's wage will increase the total level of corruption. Navot et al. (2016) argue that the positive relationship between the wage of public officials and their corruptibility is linked to their incentives and motivation to serve the public. First, the authors claim that higher wages may attract more selfish officials with higher pecuniary incentives, who are more tempted to exercise opportunities that benefit them, and thus have a higher probability of behaving corruptly when they have an opportunity to do so. Second, Navot et al. (2016) point out that officials may perceive the increase in wage as an attempt to control their behavior. According to Ostrom (2005), this negatively affects the self-determination and self-esteem of such officials, thus reducing their intrinsic motivation to serve the public and in turn increase their propensity to become corrupt. The third camp finds some evidence in favor of a non-linear relationship between wage and corruption. Prominently, Schulze, Sjahrir and Zakharov (2016) and Chen and Liu (2018) find empirically that the association between officials' wages and corruption seems U-shaped:

[5] The authors observed the increase in the officials' corruptibility only after the relative wage reached a certain threshold.

Corruption decreases with the relative wage at low or medium levels but starts increasing again after the wage becomes sufficiently high. Momotenko (2019) finds a similar U-shaped pattern of the propensity to reciprocate a bribe in an experiment based on a bribery game. This experiment consists of four treatments and officials' relative wage increases with each treatment. I find that while the bribe acceptance rate decreases significantly with the relative wage[6], the propensity to reciprocate a bribe decreases with the relative wage until it reaches two (in the first three treatments) and then starts increasing again (in the last, fourth, treatment)[7]. Such a U-shaped wage-corruption relationship could explain the contradictory evidence found by the first two camps of literature. Neither Schulze et al. (2016) and Chen and Liu (2018) nor Momotenko (2019), however, provide an intuition behind such a pattern.

SSES theory, which I test in the present study, fits the findings of Schulze et al. (2016), Chen and Liu (2018) and Momotenko (2019), and could provide an intuition behind the U-shaped wage-corruption relationship: When the relative wage is at a low or medium level, officials' behavior is motivated by the feeling of reciprocity (job appreciation) and economic considerations captured by the efficiency-wage theory. When, however, the relative wage is high, SSES increases, which increases corruptibility.

My approach improves on previous research in several ways. First, by using the experimental approach, I am able to observe the effect of the increase in SSES on corruptibility in a controlled environment which is relatively free of the confounding factors one may face in the field[8] (see Dusek, Ortmann, and Lizal, 2005; Abbink, 2006; Frank, Lambsdorff, and Boehm, 2011; Armantier and Boly, 2013). This helps to produce more reliable results than those obtained by studies using observational data which, as I will show in Section 3.2.1, possibly suffer from measurement errors and other confounds. Unlike previous experimental studies on the relationship between SSES and anti-social behavior, I use a more robust version of the experimental bribery game, with an additional earnings stage where the participants earn their initial endowments by taking a test. This game fits the real-life corruption experience well and thus tends to increase the

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[6] Relative wage is defined as the official's wage divided by the firm's wage in this experiment.
[7] … further research is needed to confirm this pattern in treatments with even higher relative wages than in the fourth treatment.
[8] Of course, experimentation itself has its fair share of challenges and confounds that need to be duly addressed; see, for example, Armantier and Boly, 2008; Dusek, Ortmann and Lizal, 2005; and Abbink, 2006.

external validity of the conclusions I draw compared to the studies using other games for the same purpose.

The results I report below provide evidence that SSES may contribute to an increase in the propensity to accept and reciprocate bribes for officials whose relative wage exceeds two,. but only if an offered bribe is big enough. Interestingly, for the worst-paid officials, those whose relative wage is one, the effect of SSES is opposite: Treated officials who underwent SSES manipulation accept and reciprocate bribes significantly less often than untreated ones. This, possibly, is related to the decrease in self-confidence which these officials might have experienced after SSES manipulation. In the following sections, I describe the proposed experiment and its design in detail.

3.2 Literature review

A number of papers have studied the link between social class and antisocial behavior. Kraus, Cote, and Keltner (2010), for example, suggest that upper-class individuals are more anti-social as they are less empathic than lower-class ones. Raine and Venables (1984) in their study on how an adolescent's heart rate influences his/her antisocial behavior, discover that only individuals with high SSES tend to behave antisocially. However, Dabbs and Morris (1990), while studying how testosterone influences antisocial behavior of men, obtained the opposite results: The risk ratios for adult delinquency and hard drug use are found to be twice as great among individuals with low SSES than among those whose SSES were higher. It is difficult, however, to generalize the results of these studies due to the specific subject pools that they use (only adolescents in Raine and Venables, 1984; only men in Dabbs and Morris, 1990). For example, the fact that social class does not affect anti-social tendencies among adolescents in Raine and Venables (1984) does not necessarily mean that the effect will remain when adults are considered: The level of education and income of adults are different from those of adolescents, which may significantly influence their willingness to be involved into unethical behavior.

Piff et al. (2012) made an attempt to study the effect of social class on antisocial behavior for a more general subject pool. They conducted five experiments aiming at establishing the causal relationship between SES and unethical behavior. Improper experimental design, however, as well as use of deception in some of the experiments may render their results unreliable. In order to show this it is necessary to look more closely at these experiments.

The first two experiments are field studies that test the difference in driving behavior of lower- and upper-class individuals. The authors investigate whether drivers of more expensive vehicles – taken to be a proxy for SES -- tend to cut off other drivers on the road (in the first study) and pedestrians at a crosswalk (in the second study) more often than drivers of less expensive cars. I shall call the drivers of more (less) expensive cars upper-class (lower-class) drivers below. Both studies conclude that while controlling for the time of the day, driver's perceived age and sex, and amount of traffic, the upper-class drivers are significantly less ethical than their opponents. However, since the authors could not control for some important confounds, this conclusion does not mean that there is an effect of social status on anti-social behavior. The recent literature shows, for example, that income is a good predictor of risk aversion: individuals with higher income tend to be significantly less risk-averse than those with lower income (among others see Shaw, 1996; Hartog, Ferrer-i-Carbonell, and Jonker, 2002; Guiso and Paiella, 2008). A different attitude to risk, in its turn, may explain the difference in the driving behavior of the upper- and the lower-class individuals. However, Piff et al. (2012) omit this variable in their analysis, thus probably biasing the results of the first two studies. Another alternative explanation for more anti-social behavior among the upper-class drivers which Piff et al. (2012) observed may be the fact that the more expensive cars are believed to be safer than the cheaper cars, thus the driver of such a car may feel less cautious on the road and pay less attention to signs, other cars or pedestrians. Hence, the results of these two experiments of Piff et al. (2012) ought to be considered with skepticism.

The third and the fourth studies examine, using a questionnaire, how the social class (both actual and self-perceived) influences tendencies toward unethical behavior. The questionnaire consisted of eight hypothetical scenarios describing a person being involved in different types of unethical behavior. The participants in this study were asked to state the likelihood of them being engaged in the same behavior. However, due to the usually illegal or immoral nature of unethical behavior people tend to lie about their own involvement in such behavior (Dusek, Ortmann, and Lizal, 2005). This especially applies to the lower-class individuals, who tend to have higher risk

aversion (Sosa, 2004) and a contextualist (focused on other individuals) orientation, which implies their higher necessity to be liked and accepted by the social group (Kraus and Piff, 2012). Thus, it is reasonable to assume that lower-class participants may be more willing to conceal their unethical behavior than their wealthy opponents, who are likely to have an opposite, solipsistic (or individualistic), orientation. This may undermine the reliability of the results of these two studies by Piff et al. (2012).

The fifth experiment employs the "game of chance" to study an individual's tendency to cheat. In this experiment, which was conducted as an online study for a chance to win a monetary prize, a special software "rolled" a six-sided die five times and each time the resulting number was displayed on the participant's screen. The participants were then asked to sum up these five numbers and report to the experimenter. Individuals' payoffs depended on the numbers they reported in the following way: $50 was paid for every five points rolled and the remaining points were rounded up or down to the nearest multiple of five. The participants were told that the rolls were random and completely anonymous. In fact, the rolls were predetermined and all participants saw the same numbers, which at the end summed up to 12. The difference between 12 and the reported score was interpreted as one's willingness to cheat. Individuals were also asked to report their socio-economic status using the MacArthur Scale of subjective SES (Adler et al., 2000). The results of this experiment suggest that social status is positively correlated with reporting a score higher than 12 or, in other words, lying. The results of this experiment, however, are not very useful for my study of corruption. Lying and behaving in a corrupt manner are different notions and may not be easily interchangeable. While the former is perceived by society and church as immoral and a violation of a social norm, the latter is not always interpreted as such. Cabelkova (2001), for example, claims that in highly corrupted societies people tend to perceive taking or giving bribes as a normal way of making deals. Therefore, the dependence between SSES and lying may not be the same as that between SSES and corruptibility. Also, in this experiment, Piff et al. (2012) intentionally provided participants with misinformation (in other words, deceived them) which, according to Hertwig and Ortmann (2008), tends to "raise participants' suspicions, prompt second-guessing of experimenters' true intentions, and ultimately distorts behavior and endangers the [experimental] control it is meant to achieve" (p. 59). Finally, a similar experiment by Suri, Goldstein, and Mason (2011) leads to the opposite conclusion. Thus, the significance of the effect of one's social status on his/her tendency to behave in a corrupt manner remains unclear.

3.3 Experimental design

3.3.1 Experimental test-bed

The objective of my experiment is to improve upon the existing studies and examine the effects of SSES on the willingness to behave in a corrupt manner. Overall, it consists of five treatments: four control treatments previously reported in Momotenko (2019) and one additional experimental treatment (Fig. 3.1 below; Treatment 5). In the four control treatments I increased the initial endowment of officials in a between-subject design and looked at the effect of a wage increase on the change in the acceptance rate of the participants. I refer to the design in

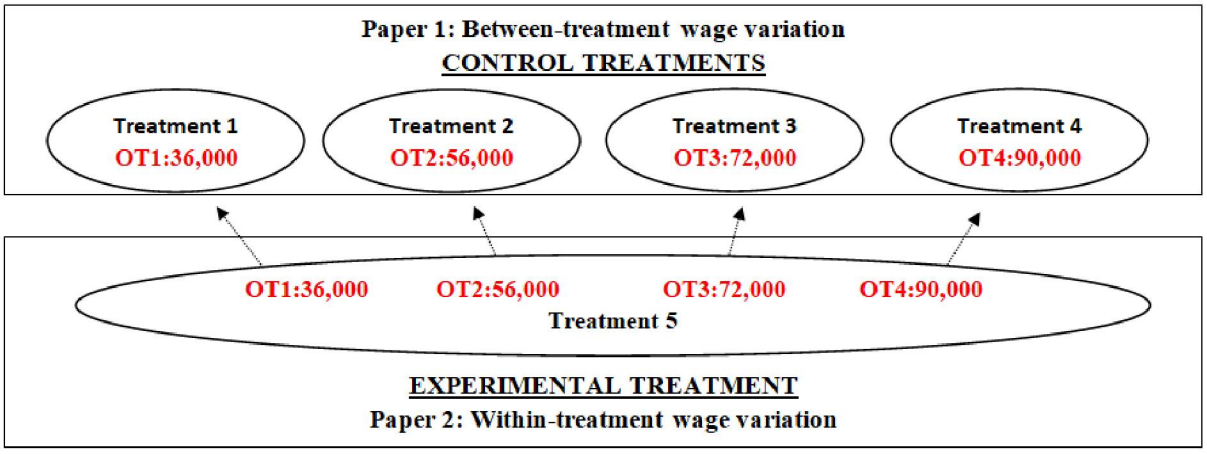

Figure 3.1: Comparison of treatments: Paper 1 – Momotenko (2019), Paper 2 – the present study, OT1-OT4 – official of type 1-4, 36,000-90,000 – officials' initial endowment

Momotenko (2019) as a between-treatment wage variation (Fig. 3.1; Paper 1), as officials' wages varied between different treatments, but were the same within a treatment. The experimental treatmentis described in this paper is identified as Treatment 5 in Fig. 3.1. I call it the Experimental

Treatment (as opposed to the Control Treatments). In Treatment 5, different officials are given different wages and are informed about the wages of other officials. For this reason, I refer to the design of this experiment as a within-treatment wage variation design[9]. In Treatment 5, I manipulate officials' perceptions of their self-perceived social class by activating higher or lower social-class mindsets. Finally, in order to excavate the effect of SSES on willingness to exhibit corrupt behavior, I compare the choices (the propensity to accept bribes and to choose option Y) of the officials from the experimental treatment with the choices of the officials from the corresponding (with the same initial endowment) control treatment. The difference in the behavior of officials in the control and experimental treatments, conditional on the wage-corruption findings, will indicate the effect of an individual's SSES on her corruptibility.

The four control treatments in Momotenko (2019) consisted of two stages: An Earnings stage and a Bribery-game stage. The experimental treatment in the current manuscript (Treatment 5 in Fig. 3.1) differs from the four control treatments in two ways: First, unlike the control treatments (which have between-treatment wage variation), the experimental treatment exploits within-treatment wage variation; second, it has an additional SSES-manipulation stage between the Earnings stage and Bribery-game stage.

During the Earnings stage, which is the same in all five treatments, the participants 'earn' their roles and initial endowments corresponding to those roles, by answering a set of questions[10]. Their respective earnings are given to them during the Bribery-game stage of the experiment. The design of the Earnings stage is similar to the one used in Momotenko (2019). A novel aspect of my method is the way questions are distributed among the participants. Each participant randomly gets one of three sets of questions of low, medium, or high level of complexity. The participants are not told, however, that the sets are different[11]. At the end of the Earnings stage the participants are ranked according to their performance in the Earnings stage (number of correct answers)[12] and

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[9] Here I talk about the design of the fifth treatment and not the experiment as a whole. In general, the experiment has between-subject design.

[10] See Appendix 3 A for the sample questions used in the Earnings stage.

[11] In the instructions distributed to the participants I state that they have 10 minutes to answer their set of questions (see Appendix 3 B). I do not provide misleading information and, according to the widespread agreement among researchers, not disclosing all experimental conditions is not considered deception. For full discussions of what deception is refer to Hertwig and Ortmann (2008).

[12] If two or more participants answer the same number of questions correctly the computer will randomly determine the rank order among them.

then the roles of Firm and Official are assigned to them as follows: Out of 16 people participating in a session, those with the worst performance and thus lowest ranks '1' - '8' are given the role of Firm; those with ranks'9' – '10' – the role of Officials of Type 1 (OT1); '11' – '12' – OT2; '13' – '14' – OT3; and '15' – '16' – OT4[13]. The initial endowments are given to participants in the manner described in Table 3.1.

Table 3.1: Initial endowments of the participants of different types

Participant's type	Firm	OT1	OT2	OT3	OT4
Wage, W	36,000	36,000	56,000	72,000	90,000

Momotenko (2019) argues that this method of assignment into roles helps to decrease[14] self-selection into roles based on abilities as the sets of questions have different levels of difficulty, are assigned to the participants randomly and are designed in a such way that an "easy" set is very likely to be answered correctly and a "difficult" set is very likely not to be answered correctly regardless of the participant's abilities. In the Results section I provide some statistical evidence in favor of this claim. At the same time this method improves upon those using windfall endowments, i.e., initial endowments being distributed randomly among participants. Since the participants are not informed that the sets of questions are different for different participants, they are likely to induce the sentiment that the assigned roles, and the initial endowment which comes with a role, are deserved. According to previous studies, this sentiment (of entitlement to the money) makes participants more selfish, as it is less affected by fairness/unfairness or reciprocity considerations that are present when the endowment is randomly granted by the experimenter (Momotenko, 2018; Cherry, Frykblom, and Shogren., 2002).

[13] This allocation is determined by two factors. First, as I want to induce a feeling of uniqueness and self-importance (to increase SSES), there should not be too many Officials in each category. Second, I need an equal number of Officials and Firms in order to form pairs.

[14]This method cannot eliminate the self-selection bias completely as more able participants are still expected to answer more questions than their less able colleagues. The ability, however, does not tend to be the main determinant of a participant's performance; I present some confirmatory evidence in the Results section.

During the SSES-manipulation stage, which is only present in the experimental treatment (Treatment 4 in Fig. 3.1), after the participants have learned their roles and initial endowments, I manipulate their perception of own socio-economic status. This manipulation becomes possible because the participants "earned" their roles and initial endowments and thus think that they are fair. Specifically, I adopt an adapted version, based on the MacArthur Scale of subjective SES (Adler et al., 2000), of the method used to activate higher or lower social-class mindsets (see Appendix 3 B for the detailed instructions). The participants acting as officials are presented with an image of a ladder with five rungs representing where participants stand in the game in terms of their income (initial endowment)[15]. They are told that those with the least money are placed at the bottom of the ladder and those with the most - at the top –. They are then asked to rank their position in the socio-economic hierarchy and to ponder how they differ from others in terms of their income. Thereafter the participants are instructed to answer a few questions and write a small essay describing the typical life of a real person in the same position on the social ladder the participant holds in the experiment: what goods they buy, what recreational activities they have, etc. Participants who place themselves higher on a ladder are expected to have higher SSES. A similar method[16] was effective for manipulation of SSES in the experiments of Piff et al. (2012), Kraus et al. (2010) and Piff, Kraus, Côté, Cheng, and Keltner (2010). During the third stage, the subjects play the Bribery game described in Section 3.3.2.

3.3.2 Bribery game

In my experiment, I use a variant of the bribery game of Van Veldhuizen (2013); it was also used in Momotenko (2019) and is described there in more detail. This two-player sequential bribery game features firms and public officials. Fig, 3.2 above depicts the stage game and the payoff structure. The stage game is repeated for twenty periods. At the beginning of each period

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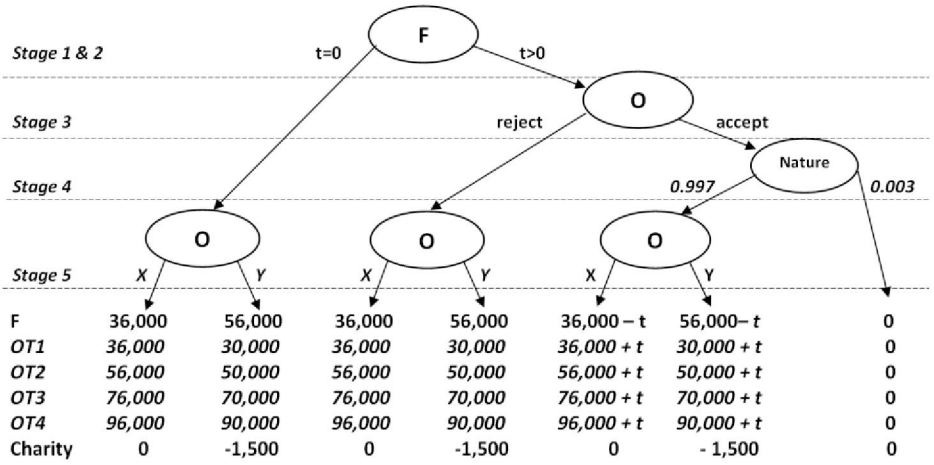
[15] The participants acting as Firms received another set of neutral questions unrelated to the experiment (see Appendix 3 B). This is done so that all the participants were occupied by the task but only the behavior of Officials was affected by the manipulation of SSES.

[16] In the experiments of Piff et al. (2012), Kraus et al. (2010) and Piff et al. (2010) the participants had to state their real-life SSES, while in my experiment they described the one they were assigned in the experiment.



F	36,000	56,000	36,000	56,000	36,000 – t	56,000– t	0
OT1	*36,000*	*30,000*	*36,000*	*30,000*	*36,000 + t*	*30,000 + t*	0
OT2	*56,000*	*50,000*	*56,000*	*50,000*	*56,000 + t*	*50,000 + t*	0
OT3	*76,000*	*70,000*	*76,000*	*70,000*	*76,000 + t*	*70,000 + t*	0
OT4	*96,000*	*90,000*	*96,000*	*90,000*	*96,000 + t*	*90,000 + t*	0
Charity	0	-1,500	0	-1,500	0	- 1,500	0

Figure 3.2: Experimental game tree: F – firm; OT1 - OT4 – official of type 1 - 4; t – a bribe.

participants are endowed with the initial endowment they earned during the earnings stage[17]. Every period in this experiment consists of five additional stages. During Stage 1, a firm decides on whether to send a bribe ("transfer t") to an official in order to entice the latter to choose the outcome which is favourable for the firm. If the firm decides not to send a transfer, then the period continues with Stage 5. If the firm chooses to send a transfer, in Stage 2 it decides on how much to send. The amount can be any integer number between 1 and the firm's initial endowment. In Stage 3 the official decides whether to accept the transfer. If the transfer is not accepted the period continues with Stage 5. If the transfer is accepted, then in Stage 4 with probability p, p = 0.003, both parties will be caught and punished. Specifically, both parties lose their earnings (except for the show-up fee) and are disqualified from the experiment. The punishment is meant to mimic the real-life experience of being fired from a job and/or being jailed for corruption. With probability (1- p) (or 0.997) both players stay in the game and the period continues with Stage 5 wherein the official chooses one of two alternatives, X and Y. When X, the neutral option, is chosen, the official does not provide a service to the firm. In this case, if the transfer was not initially offered or offered and refused, both firm and official get a payoff equal to their initial endowments. If, however, the transfer was offered and accepted, the participants were not disqualified and option X is chosen, the firm's/official's payoff is the initial endowment decreased/increased by the amount of the

[17] For public officials this money represents their wages and for the firms - their profits.

transfer. An alternative option, option Y, is the corrupt option: the official provides the service to the firm and it obtains the benefits.

When the corrupt service is provided, the official endures costs. These are the costs associated with applying effort to either provide a corrupt service or to justify her choice to colleagues and superiors or both. So the official strictly prefers option X to option Y. Importantly, when the corrupt option Y is chosen a substantial sum of money is deducted from the third party, which in the context of this experiment is represented by a charity. This mimics the negative externalities of corruption which are imposed on the society. The more corrupt deals occur, the more money is deducted from the amount reserved for the charity at the beginning of the experiment and the less money is donated at the end of the experiment. The firm, however, favours option Y. After all the decisions are made, the payoffs are realized.

The parametrization of the stage games is similar to that in Van Veldhuizen (2013)[18] (Fig. 3.2). The values of initial endowments W vary across the treatments (Table 3.1). Initial endowments of officials of type 1 and 2 (OT1 and OT2) are the same as those of Van Veldhuizen (2013), and for OT3 and OT4 they are set such that they correspond to 2 and 2.5 relative wages to meet the values of the relative wages in Schulze et al. (2016).

The subgame-perfect Nash equilibrium of the stage game is for the official to always choose option X and for the firm to never make a transfer. This equilibrium is preserved in a finitely repeated game: In the last period the official always chooses option X and which means the firm never offers a bribe. Theory thus predicts a non-corrupt society. With my experiment, I aim at testing this theoretical prediction as well as the two hypotheses stated below.

3.3.3 Hypotheses

The experimental game described is meant to test the following two hypotheses.

[18]Van Veldhuizen (2013) sets W equal 36 and 56. I decided to scale up these numbers in order to make them correspond roughly to the real-life wages of public officials in the Czech Republic (according to the Informational system on Average Earnings (ISPV) the average monthly wage of a public official in 2015 was around 30,000 CZK).

Hypothesis 1: An increase in officials' SSES leads to an increase in the probability of accepting a bribe when an official's relative wage is high enough.

I formulate a similar hypothesis for the probability of acting on the bribe (in my experiment, choosing option Y).

Hypothesis 2: The increase in officials' SSES leads to an increase in the probability of choosing option Y when an official's relative wage is high enough.

3.3.4 Experimental procedure

The experimental procedures for the four control treatments are described in Momotenko (2019). Here, I shall describe the procedure for the experimental treatment (Treatment 5 in Fig. 3.1). I ran all sessions of this treatment during March 22-28, 2018 at the Laboratory of Experimental Economics (LEE) at the University of Economics in Prague using zTree software (Fischbacher, 2007). Subjects were recruited through an Online Recruitment System for Economic Experiments (Greiner, 2015).

I recruited 160 subjects for data collection in Treatment 5 (10 sessions of 16 subjects)[19]. The age of the participants ranged from 19 to 41 (with a median of 23), with 35% mentioning Economics as their major.

Before entering the laboratory, the participants were randomly assigned to their seats where they were given printed instructions after being seated. The instructions for the experiments (see Appendix 3 B) consisted of two parts, which were distributed separately: The first part of the instructions was given to the participants immediately and the second part was distributed after everyone completed the earnings stage of the experiment. Both parts of the instructions were written using neutral terminology (transfer instead of bribe, Participant A and B instead of Firm and Official, etc.).

[19]Momotenko (2019) also employed 160 subjects for the data collection in the control treatments; thus in total, there were 320 subjects.

During the earnings stage the participants had ten minutes to complete the test consisting of ten questions (see Appendix 3 A). After completing the test, they learned their roles (the roles stayed constant during all periods of the experiment) and were given the second part of the instructions, which described the bribery game in detail. After they finished reading the instructions, the participants were asked several comprehension questions (see Appendix 3 C) and went through two trial periods of the game before the beginning of the payoff-relevant periods to make sure everyone understood the instructions. The printouts of the instructions stayed with the participants and they could refer to them any time during the experiment. After answering comprehension questions, the subjects were asked to choose a charity for the experiment from the list of five charities well-known in the Czech Republic: UNICEF, Red Cross, Greenpeace, The Catholic charity Caritas Czech Republic, and People in Need (Člověk v Tísni)[20]. At the end of the session, the computer decided which charity out of those chosen by the participants would receive the donation[21]. A total of 900 CZK[22] (approximately 35 Euros) was reserved for the charity at the beginning of the experiment.

Once the participants had chosen a charity, the participants acting as Officials were instructed (the instructions were on their computer screens) to answer questions from the SSES-manipulation stage. The participants acting as Firms were asked to answer a set of irrelevant (for this experiment) questions in order to occupy them with an activity while the Officials were completing the SSES-manipulation stage. After this stage and two trial periods, the experiment proper started. It consisted of twenty periods. At the beginning of each period the participants were given the initial endowment corresponding to their roles. Pairings remained fixed for all periods. At the end of each period, participants learned their payoffs for it. Pairs which were disqualified were notified accordingly by a message on their screens but continued making decisions until the

[20] These are the most popular and well-known charities in the Czech Republic and cover a wide range of interests: promoting human rights (People in Need), social and health care (The Catholic charity Caritas), environmental issues (Greenpiece), interests of victims of armed conflicts (Red Cross) and children's rights (UNICEF). But even if a participant was reluctant to donate to any of the proposed charities, the fact of hurting a real-life charity should have added a negative connotation to making a corrupt choice.

[21] Each participant had the same probability of being selected by the computer.

[22] This amount mimics that of van Veldhuizen (2013), on which the experiment of Momotenko (2019) drew, but is adjusted to the difference in the level of prices between Amsterdam (where van Veldhuizen's experiment was conducted) and Prague (CPI equalled 34%).

end of the experiment[23]. At the end of twenty periods one period was randomly selected for cash payment[24]. The money deducted from the charity depended on the number of times option Y was chosen during the whole session by all pairs. The winning charity was selected randomly by the computer (using a random number generator). Participants were then asked to fill in a questionnaire and were paid their endowments in a separate room converted into the local currency (CZK) according to an exchange rate of 10,000 Talers (experimental currency units) to 45 CZK[25].

The average cash payoff of participants was 324 CZK (12.6 Euros[26]) (including the show-up fee of 100 CZK). The charities were given on average 642 CZK (out of 900 CZK or 25 Euros out of 35 Euros, for a social welfare loss of 258 CZK or 10 Euros). The anonymous donations to the winning charities were made online after each treatment and the payment confirmations were sent out to the participants in the corresponding session.

3.4 Results

[23] This was needed to prevent disqualified participants from revealing themselves to others and thus possibly affecting other participants' perceptions of the probability of being caught and punished and their consequent choices in the experiment. During all sessions of the experiment neither of the disqualified participants revealed themselves in any way and they only got a show-up fee at the end of the experiment as was stated in the instructions.
[24] Following Momotenko (2019), and widely established precedent, I chose to pay for one randomly chosen period instead of paying cumulative earnings for the whole session, in order to incentivize participants to think carefully about their decision in each period and independent from the (possible) outcomes of the (future) previous periods. This method is used by, for example, Charness and Genicot (2009) and Fischer (2013).
[25]The exchange rate mimics that of van Veldhuizen but is adjusted to the difference in the level of prices between Amsterdam (where the experiment of van Veldhuizen was conducted) and Prague (CPI equals 34%) and to the fact that the final payment was not the cumulative sum of earnings for twenty periods but a payoff from one randomly chosen period.
[26] Exchange rate 1 Euro = 25.7 CZK was used

In this section I present and analyze the results of the experiment. I shall first describe the results of the earnings stage; then I shall separately analyze the results of bribe acceptance and the propensity of choosing corrupt option Y versus non-corrupt option X.

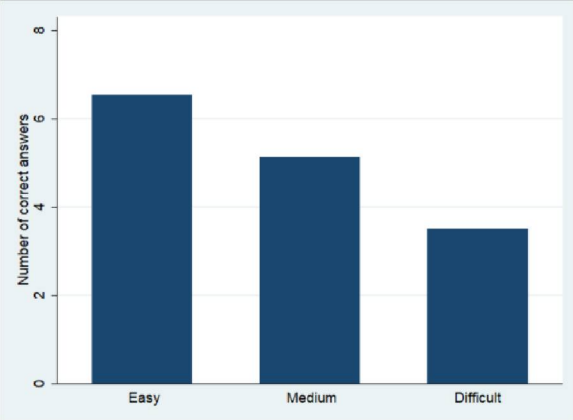


Figure 3.3: Average number of correct answers over different sets of questions during the earnings stage of the experiment[27]

The intention of the earnings stage was to make the participants believe that the random distribution of roles and endowments was fair. A comparison of average numbers of correct answers over different sets of questions show that the number of test questions answered correctly decreases with the difficulty of the set of questions (Fig.3.3). The results of the regression of the number of correct answers on the set of questions and personal characteristics such as age, gender, maximum earned degree and major, also suggest that the set of questions is the only significant determinant with a p-value of 0.011 (coefficient -.0697). Thus, I conclude that the randomization worked.

When looking at the main results, contrary to the subgame-perfect prediction of non-transfers, in the majority of pairs (65 out of 80) positive transfers were made at least once. The median number of periods a positive transfer was offered is equal to 12 (out of 20). The majority of pairs also chose option Y at least once (in 58 pairs out of 80). The median number of periods option Y was chosen is equal to 4.

[27] Six easy, five medium and five difficult sets of questions were divided between sixteen participants in a session. This proportion (6:5:5) stayed the same in all sessions.

For the analysis of the main results I exclude seven official-firm pairs in which a bribe was never offered, since to study an officials' corruptibility a bribe should be offered to her at least once during the experiment. I also exclude three pairs which were disqualified and one pair in which either of the partners did not answer any of comprehension questions correctly. Ultimately, 69 official-firm pairs[28] were left for the further analysis. I use a between-subjects design to test the stated hypotheses.

3.4.1 Analysis of SSES manipulation

In order to check if SSES manipulation worked, I analyse participants' answers to the questions of the SSES manipulation stage and their essays. The results of the multinomial logit regressions show that people with higher SSES would buy goods of higher quality, would have better recreational activities, would shop in more luxurious shops and would have more expensive accommodation (see Appendix 3 E Table 1 and Table 2 for description and summary

Table 3.2: Average bribe acceptance rate over wage groups

Wage group	Control treatments	Experimental treatment
OT1	72%	76%
OT2	65%	67%
OT3	71%	54%
OT4	65%	44%

statistics of the variables). They also suggest that SSES does not significantly influence the choice of transportation. I conjecture that the reason for this finding is that in Prague (where the experiment was conducted) public transport is a better alternative to other means of transportations

[28] The current study is not properly powered up (e.g., Zhang and Ortmann 2013) although the sample size is in line with rules of thumb presented in List et al. (2011). A key reason is budget constraints.

due to its convenience (avoidance of frequent traffic jams) even for those who can afford expensive cars.

In the essays where participants described how the interaction with people of the highest and lowest ranks would go, 41 % (the biggest group[29]) of the participants with the lowest SSES (SSES equal 2) indicated that they would feel uncomfortable, shy and distant when communicating with those of the highest rank and 94 % stated that they would feel comfortable, sure and relaxed when communicating with those of lowest rank. When asked the same question, the participants with the highest SSES (SSES equal 5) indicated that they would feel sure, relaxed and comfortable when communicating with people from either of the ranks (56% when. communicating with highest rank and 94% with the lowest rank). These results suggest that the SSES manipulation helped to enhance the feeling of superiority among OT4 officials and feeling of inferiority among OT1 officials, as implemented by the earnings stage of the experiment.

3.4.2 Analysis of bribe acceptance

In order to check if there is an effect of SSES on the propensity to accept bribes, I compare the number of accepted bribes by the officials from a certain wage group in theexperimental treatment, Treatment 5, to the officials from the corresponding (having the same initial endowment) group in the control treatment. The main source of identification is whether there was SSES manipulation in a treatment.

Hypothesis 1 suggests that the increase in officials' SSES leads to an increase in the probability of accepting a transfer when the relative wage exceeds 2 (for OT4). In order to check this, I first compare the average number of bribes accepted by officials in each wage category (Table 3.2). Contrary to my prediction the acceptance rate for OT4 is 21 percentage points lower in experimental treatment compared to the corresponding control treatment (Treatment 4). Thus, the comparison of the means does not support Hypothesis 1. In order to draw a conclusion, I then check the results of the probit regression of the decision to accept a transfer on treatment dummy

[29] The rest indicated that they would be either indifferent or feel comfortable in this situation.

Table 3.3: Probit estimates for bribe acceptance for all wage groups. Between treatments comparison

	Dependent variable: bribe accepted (1 = yes)							
	(OT1)		(OT2)		(OT3)		(OT4)	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Treatment5	-.038	-.118*	.0263	-.0344	-.226***	-.244***	-.0966	-.102
	(.057)	(.068)	(.048)	(.056)	(.0515)	(.0607)	(.0727)	(.0882)
Male	-.153***	-.141**	.039	.0308	.0385	.0478	-.163***	-.193***
	(.057)	(.069)	(.059)	(.0632)	(.0588)	(.0674)	(.0592)	(.0733)
Age	-.002	-.003	-.006	-.0139	-.0405***	-.0483***	-.0312**	-.0206
	(.005)	(.006)	(.0111)	(.0137)	(.0146)	(.0174)	(.0149)	(.0173)
Economics student	.025	.082	.0986*	.11	.0167	.0175	.142**	.173**
	(.052)	(.058)	(.0546)	(.0678)	(.0585)	(.0644)	(.0586)	(.0679)
Risk	.049***	.053***	.0153	.0134	.022*	.0273**	.0443***	.0572***
	(.008)	(.009)	(.0122)	(.0137)	(.012)	(.0137)	(.0159)	(.0189)
Highest degree	-.001	.004	.0748	.146**	.304***	.382***	.132*	.0821
	(.038)	(.040)	(.0556)	(.0671)	(.0602)	(.0698)	(.0798)	(.101)
Periods	All	5 to 20	All	5 to 20	All	5 to 20	All	5 to 20
Observations	344	230	408	293	358	269	297	211
Conditional on a transfer having been proposed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of eight probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only
* Significant at 10%, ** Significant at 5%, *** Significant at 1%.

Table 3.4: Probit estimates for bribe acceptance if t >7000 Talers for all wage groups. Between treatments comparison

	Dependent variable: bribe accepted (1 = yes)							
	OT1		OT2		OT3		OT4	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Treatment5	-.0852*	-.136***	-.0171	-.0896*	-.163**	-.257***	.2169*	.285*
	(.0454)	(.0528)	(.0496)	(.0515)	(.0655)	(.0795)	(.112)	(.154)
Male	-.136***	-.122***	.0165	-.000313	-.00894	.0616	-.0964	-.170**
	(.0468)	(.0462)	(.0616)	(.0652)	(.0689)	(.0780)	(.0692)	(.0860)
Age	-.00584*	-.00381	-.0328**	-.0524***	-.0428***	-.0585***	.00914	.0151
	(.00324)	(.00297)	(.0135)	(.0164)	(.0154)	(.0178)	(.0375)	(.0363)
Economics student	.0103	.0348	.0291	-.0338	.0115	.0272	.390***	.412***
	(.0403)	(.0370)	(.0566)	(.0663)	(.0698)	(.0776)	(.109)	(.116)
Risk	.0367***	.0346***	.0209	.0269**	-.0162	-.0232	.0367*	.0326
	(.00681)	(.00741)	(.0131)	(.0132)	(.0155)	(.0170)	(.0223)	(.0261)
Highest degree	.0282	.0134	.146**	.258***	.208***	.314***	.115	-.0686
	(.0305)	(.0277)	(.0587)	(.0658)	(.0745)	(.0880)	(.126)	(.160)
Periods	All	5 to 20	All	5 to 20	All	5 to 20	All	5 to 20
Observations	248	172	265	200	206	155	149	112
Conditional on a transfer having been proposed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of eight probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use robust standard errors. The regressions use the data for public officials only
* Significant at 10%, ** Significant at 5%, *** Significant at 1%.

and personal characteristics I run probit regressions for each wage group (OT1-OT4) and use a specification[30] with all controls[31]. I run this model with (model a) and without (model b) the first five periods of the game to account for learning effects. The results are summarized in Table 3.3. I predicted the increase in the acceptance rate for officials of type OT4 after SSES manipulation. The model OT4, however, rejects this prediction: The coefficient for the main variable of interest, a dummy for the experimental treatment, Treatment 5, is negative and insignificant in both cases: (a) and (b). In this model, the main determinants of whether the bribe is accepted are gender, studying economics and being riskier. Interestingly, the propensity to accept bribe among the worst-paid officials decreases after SSES manipulation and this decrease is weakly significant in model (b).

I further check if this result holds regardless of the amount of bribe offered to officials. It is possible that treated officials choose to accept more bribes but only if an offered bribe is big enough. Indeed, Table 3.4 shows that when the bribe is higher than 7000 Talers, the effect of SSES manipulation on the probability of accepting a bribe for officials with the highest wage, OT4, is positive and significant. This suggests that officials tend to accept more bribes if they are on the top of the social ladder and the bribe is high enough. In contrast, the SSES effect on officials with the lowest wage, OT1, is negative and significant. Thus, I cannot reject Hypothesis 1 when the bribe exceeds 7000 Talers but cannot accept it otherwise.[32]

Finally, to conclude the analysis of officials' behavior I check if the best-paid officials are less likely to reject smaller transfers after SSES manipulation, which also signals that they are more inclined to unethical behavior. Indeed, OT4 officials tend to reject smaller offers significantly less often after SSES manipulation than in the corresponding control treatment[33]. On the contrary,

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[30] In Momotenko(2018), the author considered three specifications: with no controls, with a reduced set of controls and with an enlarged set of controls. The last specification was chosen as the preferred one.
[31] The questionnaire used to elicit the data for controls can be found in Appendix 3 D. The description of all variables and summary statistics are described in Appendix 3 E.
[32] I understand, however, that the study may be underpowered and running more experimental sessions would be needed to get more reliable estimates of the effects.
[33] In the robust regression of the amount rejected on the treatment dummy and personal characteristics the coefficient before dummy treatment is (-1851.3) and p-value is (0.026).

OT1 officials tend to reject smaller offers more often after SSES manipulation and this difference is weakly significant[34].

3.4.3 Analysis of the number of Y choices

In order to test Hypothesis 2, I compare the number of times Option Y was chosen by officials from a certain wage group in the experimental treatment, Treatment 5, and a corresponding control treatment. The main source of identification is whether there was SSES manipulation in a treatment.

Table 3.5: Average number of Y choices over wage groups

Wage	Control	Experimental
OT1	52%	47%
OT2	40%	35%
OT3	29%	27%
OT4	46%	21%

Hypothesis 2 suggests that the increase in officials' SSES leads to an increase in the probability of choosing Option Y when the relative wage exceeds 2 (for OT4). In order to check this, I first compare the average number of Y choices by officials in each wage category (Table 3.5).

Contrary to my prediction the number of Y choices for OT4 is 25 percentage point lower in the experimental treatment (Treatment 5) compared to the corresponding control treatment (Treatment 4). Thus, the comparison of the means does not support Hypothesis 2. In order to draw a conclusion, I need to check the results of the probit regression of the decision to choose OptionY

[34] In the robust regression of the amount rejected on the treatment dummy and personal characteristics the coefficient before dummy treatment is (658.1) and p-value is (0.090).

on experimental treatment dummy, Treatment 5, and personal characteristics. I run probit regressions for each wage group (OT1-OT4) and use one main specification with all

Table 3.6: Probit estimates for Y choices for all wage groups. Between treatments comparison

	Dependent variable: Option Y chosen (1 = yes)							
	(OT1)		(OT2)		(OT3)		(OT4)	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Treatment5	-0.164***	-0.330***	-0.00735	-0.0322	0.00958	0.0366	-0.000885	-0.0242
	(0.0614)	(0.0791)	(0.0514)	(0.0584)	(0.0520)	(0.0543)	(0.0737)	(0.0964)
Male	0.0509	-0.0710	0.236***	0.303***	0.0625	0.0837	0.184***	0.118
	(0.0706)	(0.0963)	(0.0620)	(0.0729)	(0.0531)	(0.0586)	(0.0658)	(0.0878)
Age	0.00549	0.00731	0.000483	0.0102	-0.0781***	-0.0941***	-0.0599***	-0.0596***
	(0.00713)	(0.00960)	(0.0127)	(0.0153)	(0.0117)	(0.0135)	(0.0158)	(0.0196)
Economics student	0.0676	0.119*	-0.141**	-0.106	0.0854	0.119**	0.182***	0.190**
	(0.0610)	(0.0702)	(0.0559)	(0.0682)	(0.0538)	(0.0562)	(0.0555)	(0.0761)
Risk	0.0336***	0.0402***	-0.0212*	-0.0260*	-0.00579	-0.00724	-0.0140	-0.00975
	(0.0110)	(0.0135)	(0.0127)	(0.0154)	(0.0117)	(0.0122)	(0.0152)	(0.0202)
Highest degree	0.0142	-0.00793	0.133**	0.135**	0.383***	0.496***	0.116	0.0931
	(0.0485)	(0.0618)	(0.0556)	(0.0637)	(0.0511)	(0.0554)	(0.0744)	(0.0976)
Periods	All	5 to 20	All	5 to 20	All	5 to 20	All	5 to 20
Observations	344	230	408	293	358	269	297	211
Conditional on a transfer having been proposed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of eight probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only

* Significant at 10%, ** Significant at 5%, *** Significant at 1%.

Table 3.7: Probit estimates for Y choices when bribe > 10000 Talers for all wage groups. Between treatments comparison

	Dependent variable: Option Y chosen (1 = yes)							
	(OT1)		(OT2)		(OT3)		(OT4)	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Treatment5	-0.267***	-0.423***	0.0837	0.0652	0.0862	0.159*	0.402**	0.395**
	(0.0959)	(0.106)	(0.0972)	(0.0984)	(0.0939)	(0.0942)	(0.176)	(0.190)
Male	-0.0131	-0.0976	0.329***	0.424***	0.00249	-0.00685	0.0255	-0.0766
	(0.107)	(0.134)	(0.0820)	(0.0743)	(0.107)	(0.114)	(0.147)	(0.165)
Age	0.000795	-0.0105	-0.0458	-0.0789**	-0.096***	-0.096***	-0.10***	-0.099***
	(0.00895)	(0.00966)	(0.0306)	(0.0313)	(0.0164)	(0.0179)	(0.0250)	(0.0307)
Economics student	0.173**	0.187**	-0.187**	-0.252***	0.353***	0.375***	0.340***	0.327***
	(0.0789)	(0.0904)	(0.0848)	(0.0854)	(0.0901)	(0.0972)	(0.0937)	(0.118)
Risk	0.0522***	0.0463***	-0.0333	-0.0391	0.0647*	0.0448	0.0248	0.0470
	(0.0144)	(0.0169)	(0.0223)	(0.0241)	(0.0262)	(0.0364)	(0.0388)	(0.0425)
Highest degree	0.0775	0.118	0.277**	0.317**	0.667***	0.603***	-0.0218	-0.0750
	(0.0660)	(0.0729)	(0.128)	(0.128)	(0.0911)	(0.0889)	(0.185)	(0.200)
Periods	All	5 to 20	All	5 to 20	All	5 to 20	All	5 to 20
Observations	177	132	134	104	118	99	69	63
Conditional on a transfer having been proposed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of eight probit regressions. The reported numbers are marginal effects; the numbers in parentheses are standard errors corresponding to these marginal effects. I use bootstrapped standard errors (1000 replications). The regressions use the data for public officials only
* Significant at 10%, ** Significant at 5%, *** Significant at 1%.

controls. I also run additional regressions, model (b), excluding the first five periods of the game to account for learning effects. The results are summarized in Table 3.6.

I predicted the increase in the propensity to choose Option Y for officials of type OT4 after SSES manipulation. The model OT4, however, rejects this prediction: The coefficient for the main variable of interest, a dummy for Treatment 5, is negative and insignificant in both cases: (a) and (b). In this model, the main determinants of whether the Option Y is chosen are gender (only in model (a)), age and studying economics. Interestingly, the propensity to reciprocate the bribe among the worst-paid officials decreases after SSES manipulation and this decrease is strongly significant. This pattern is similar to the one I observed in the propensity to accept bribes. I further check if such results hold regardless of the amount of bribe offered to officials. As in the case with bribe acceptance, it is possible that treated officials choose Option Y more often but only if an offered bribe is big enough. Indeed, Table 3.7 shows that when offered a bribe bigger than 10000 Talers, treated officials of type OT4 from the experimental treatment are around 40 percentage points[35] more likely to choose Option Y than non-treated officials from the control treatment. On the contrary, the treated officials of type OT1 are around 42 percentage points in model b) less likely to choose Option Y than officials with the same wage from control treatments when given a bribe larger than 10000 Talers.

3.4.4 Behavior of firms

As in the experiment, firms move first, their decisions on the amount and the frequency of offering a bribe influence the choices that are then made by officials. In this subsection I investigate some aspects of the behavior of firms as an attempt to better understand the behavior of officials. In the experiment, firms did not participate in the SSES stage. Thus, I attribute any change in firms' behavior to the switch from between-treatment to within-treatment wage variation design.

[35] This result should be taken with caution as the study is underpowered and more sessions need to be run in order to get more reliable conclusions

Table 3.8: Number of firms who decided not to give bribes

W of a paired official	Treatment 1	Treatment 2	Treatment 3	Treatment 4	Treatment 5
36000	1				2
56000		1			1
72000			2		2
90000				4	2

To analyze firms' behavior I first inspect if there is any pattern in the behavior of firms which never offered a transfer. There is an increase in the amount of such firms in the control treatments (from Treatments 1 to Treatment 4), which may indicate that firms anticipate better-paid officials to be less corrupt. This pattern differs from that in the experimental treatment (Treatment 5). The number of firms who decided not to give bribes increases in the experimental treatment compared to the control treatment when paired with OT1 officials and decreases (compared to the control treatment) when paired with OT4 officials (see Table 3.8). I provide two possible explanations for that difference in firms' behavior.

In the experimental treatment, firms have the lowest SES out of five different SESs. It is possible that, because firms realize that they are the worst performers, their desire to reduce the income gap between them and OT4 officials increases, which can induce more firms to engage in corrupt behavior. Another possibility is that firms anticipate the best performers, OT4 officials, to be more tactical and selfish and thus more willing to create long-term corrupt relationships and chose corrupt option Y which is beneficial to firms.

Second, I analyze firms who made a transfer at least once. In control treatments, Treatment 1-4, it seems like those firms who decided to bribe better-paid officials tried to make them act on a bribe by giving them larger and more frequent bribes. This is confirmed by the results of two regressions ran for Treatments 1-4: a robust regression of the size of the transfer on treatment dummies and controls and a probit regression of the decision to give a bribe on treatment dummies and controls (the coefficient before treatment dummies are significant, positive and increasing with the partner's wage).

A comparison of firms' behavior in the control and experimental treatments reveals a different pattern which signals the presence of an SSES-manipulation effect: In the experimental treatment, OT4 officials are given significantly smaller (see Table 3.9) and less frequent (see

Table 3.9: Regression estimates for the size of the transfer offered by firms to officials of all wage groups

	Dependent variable: Option Y chosen (1 = yes)							
	(OT1)		(OT2)		(OT3)		(OT4)	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Treatment5	1454.0***	1554.6***	105.4	-1263.4*	235.4	626.3	-1556.4***	-2011.0**
	(470)	(568.3)	(557.4)	(649.3)	(440.1)	(552.1)	(605.5)	(794)
Male	-770.4*	-781	-87.87	-344	-1419.5***	-1291.0**	-2314.0***	-2618.7***
	(459.4)	(552.8)	(527.6)	(642.7)	(441.4)	(548.5)	(545.1)	(720.5)
Age	-235.5***	-278.2***	-476.8**	-974.9***	190.9	153.7	-172.2	-422.1**
	(65.02)	(69.9)	(198.4)	(196.5)	(140)	(152.2)	(152.8)	(196.2)
Economics student	1021.4*	1521.6**	-2850.0***	-3896.2***	-28.47	227	-1094*	-971.9
	(595.1)	(740.5)	(641.5)	(718.2)	(410.5)	(510)	(609.4)	(802.8)
Risk	146.2	99.13	463.4***	521.1***	865.9***	1049.2***	-239.2*	-350.7**
	(102)	(117.7)	(106.7)	(122.3)	(105.9)	(139.3)	(125.1)	(164.1)
Highest degree	1012.1**	1123.7**	-194	776	-577.8	-577.3	31.43	1068.4
	(402.3)	(466)	(476.3)	(533.1)	(424.1)	(467.9)	(763.5)	(989.7)
_cons	8215.0***	8729.3***	15444.7***	26448.3***	-738.8	-1249.8	14252.0***	19599.0***
	(1493.5)	(1573.2)	(5020.6)	(5160.5)	(2911.5)	(3288.4)	(3607)	(4799.7)
Periods	All	5 to 20	All	5 to 20	All	5 to 20	All	5 to 20
Observations	792	540	770	525	704	480	638	435
Conditional on a transfer having been proposed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of eight robust regressions. The numbers in parentheses are standard errors corresponding to the effects. The regressions use the data for firms only.
* Significant at 10%; ** Significant at 5%.; *** Significant at 1%

Table 3.10: Probit regressions estimates for the propensity to offer a transfer to officials of all wage groups

	Dependent variable: Option Y chosen (1 = yes)							
	(OT1)		(OT2)		(OT3)		(OT4)	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Treatment5	0.179524***	0.20784***	0.011778	-0.01276	0.208385***	0.241815***	-0.01305	-0.04042
	(0.03543)	(0.041814)	(0.041349)	(0.050854)	(0.034973)	(0.039948)	(0.042472)	(0.052455)
Male	-0.06047*	-0.09713**	0.118963***	0.126255**	-0.0987***	-0.08979**	-0.10312**	-0.10967**
	(0.035953)	(0.044347)	(0.043995)	(0.051561)	(0.037184)	(0.045503)	(0.043966)	(0.051824)
Age	-0.00999*	-0.01054	-0.05823***	-0.07129***	0.016881	0.025491*	0.009333	0.001136
	(0.005485)	(0.00649)	(0.01142)	(0.013527)	(0.011)	(0.013345)	(0.01446)	(0.016612)
Economics student	0.188753***	0.201171***	-0.17574***	-0.18781***	0.037212	0.06221	-0.22646***	-0.27928***
	(0.047667)	(0.056264)	(0.041417)	(0.048284)	(0.040678)	(0.047278)	(0.040339)	(0.048846)
Risk	0.012871	0.004962	0.040621***	0.039252***	0.042646***	0.039845***	-0.0016	-0.00992
	(0.008615)	(0.010211)	(0.007661)	(0.008799)	(0.006839)	(0.008168)	(0.010788)	(0.012471)
Highest degree	0.071427**	0.071912**	0.124474***	0.169179***	-0.03784	-0.05779	-0.09998*	-0.08057
	(0.029918)	(0.034384)	(0.034903)	(0.041624)	(0.044394)	(0.051932)	(0.056576)	(0.065363)
Periods	All	5 to 20	All	5 to 20	All	5 to 20	All	5 to 20
Observations	792	540	770	525	704	480	638	435
Conditional on a transfer having been proposed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table displays the results of eight robust regressions. The numbers in parentheses are standard errors corresponding to the effects. The regressions use the data for firms only.
* Significant at 10%; ** Significant at 5%.; *** Significant at 1%

Table 3.10) bribes than OT4 officials from the corresponding control treatment (Treatment 4), though the latter difference is insignificant. There are two possible explanations for this: First, firms expected treated OT4 officials to be more selfish and unethical and thus accept even smaller bribes. Second, firms expected treated OT4 officials to accept less bribes and thus did not put much effort in trying to incline them to do so. At the same time, OT1-OT3 officials in the experimental treatment are given larger and more frequent bribes; this effect, however, is statistically significant only for OT1 and OT3 officials and only for bribe frequency.

3.4.5 Concluding discussion

Overall, the results of my experiment do not support the hypothesis of the increase in officials' corruptibility when the relative wage is high enough (though, this may be due to the experiment being underpowered). However, a few interesting conclusions can be drawn. There are three findings that may indicate that the best-paid OT4 officials become more inclined to unethical behavior after undergoing SSES-manipulation. First, I find treated OT4 officials from the experimental treatment (Treatment 5) to be more willing to accept smaller transfers than untreated OT4 officials from the control treatment (Treatment 4). Second, if an offered bribe is larger than 7000 Talers, the bribe acceptance rate is significantly higher (29 percentage point) for the treated OT4 officials than for untreated ones. Apparently, in my experiment 7000 Talers was considered as a minimum threshold for a "fair" bribe by the OT4 officials, and thus they accepted such bribes more often. This is in line with the results of the experiment of Foltz and Opoku-Agyemang (2015), where border policemen started demanding bigger bribes from the truck drivers and did so more intensively after their salary was doubled. This effect, however, disappears if bribes of all sizes are considered, which may be due to firms offering significantly smaller bribes to OT4 officials in the experimental treatment than in the control one. This may also be the reason for the similar pattern in the decision to act on a bribe (choosing Option Y), which is the third finding: Treated OT4 officials are around 40 percentage points more likely to choose Option Y than non-treated officials from the control treatment if an offered bribe is larger than 10000 Talers.

Interestingly, the propensity to accept a bribe among the worst-paid OT1 officials decreases after SSES manipulation, and this decrease is weakly significant if bribes of all kinds are studied, and strongly significant for bribes larger than 7000 Talers, and when the first five periods are

excluded. First, I check if this is connected to the increase in risk-aversion of OT1 officials after SSES manipulation as a result of their decreased confidence due to being the "worst performers" among their colleagues. A non-parametric Wilcoxon rank-sum test, however, suggests that there is no significant difference in risk-aversion of OT1 officials in the control and experimental treatments. Thus, it is possible that after SSES-manipulation, OT1 officials started to treat their low initial endowment as "deserved" and decreased their efforts in increasing the income gap between them and firms. This could also explain the finding that OT1 officials tend to reject smaller offers significantly more often after SSES manipulation and are around 42 percentage points (in model (b)) less likely to choose Option Y than those from the control treatments when offered a bribe bigger than 10000 Talers.

3.5 Conclusion

In this paper, I investigate the association between public officials' self-perceived social status and corruptibility as a possible explanation for the increase in the level of corruption of the highest paid officials. I employ a three-stage experiment to explore such a relationship. The design of my experiment helps to improve upon the disadvantages of the previous studies on this issue such as endogeneity, measurement error, or selection bias.

The SSES manipulation is based on the fact that in my experiment the participants earn their endowments and roles in the earnings stage and thus perceive them as deserved. This feeling is then intensified in the SSES-manipulation stage when participants are asked to think about their position on the socio-economic ladder in this experimemt and to compare themselves to the participants of the highest and lowest ranks.

The results show that the effect of SSES is a significant determinant of officials' propensity to accept and reciprocate bribes only if an offered bribe is high enough, and insignificant overall. This may be due to the fact that firms gave significantly smaller bribes to the highest-paid officials in the experimental treatment than to the officials of the same type in the control treatment. It is also possible that the study is underpowered and additional sessions should be run to get more reliable results.

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Appendix 3

A Sample questions for the Earnings stage

Easy questions:

Of the following, which is greater than 1/2 ?

A: 2/5; B:4/7; C:4/9; D:5/11; E: 6/13

What is the capital of Austria?

A: Tirana; B: Andorra la Vella; C: Yerevan; D: Vienna

True or false? Pi can be written as a fraction.

A: Pi is not a number; B: It depends; C: False; D: True

Difficult questions:

Gordon is twice as old as Tony was when Gordon was as old as Tony is now. The combined age of Gordon and Tony is 112 years. How old is Gordon now?

Whet is most nearly opposite in meaning to

A: Deaden; B: Engender; C: Default; D: Enhance; E: Desiccate

In the two numerical sequences below, one number that appears in the top sequence should appear in the bottom sequence and vice versa. Which two numbers should be changed round?

100, 89, 76, 63, 44, 25

105, 93, 79, 61, 45, 25

A: 61 and 63; B: 44 and 45; C: 89 and 93; D: 100 and 105; E: 100 and 93; F: 89 and 79

B Sample instructions

Instructions

Welcome to the experiment!

Please turn off your cell phone and other electronic devices now and leave them turned off for the remainder of the experiment.

General information on the experiment

You are going to participate in an experiment on decision making.

If you read the following instructions carefully, you will be able to earn an appreciable amount of money. How much you will earn depends on your, and others', decisions. It is therefore important that you understand the following instructions.

The instructions consist of two parts.

Part 2 of the instructions will be given to you after you complete Part 1.

Part 1 of the instructions follows immediately these introductory explanations.

Once you finish reading Part 1 of the instructions press the button "Continue" on the computer screen.

- **Anonymity**

In the experiment, all interactions among participants will take place through the computer network.All participants decide anonymously, i.e. the other participants will not be able to find out the decisions you make during the experiment. To ensure anonymity it is imperative that all participants observe the following rule: **During the experiment, all communication is prohibited**, i.e. you are not allowed to speak or otherwise express yourself. **Usage of any electronic device and the Internet is prohibited. If you violate these rules, you will be dismissed from the experiment and not be paid.**

- **Payments**

The currency used in this experiment is Talers. The total amount of Talers that you have earned during the experiment will be converted into CZK at the end of the experiment at an exchange rate of

10,000Taler = 45 CZK.

You will also receive a show-up fee of 100 CZK. You will be paid your earnings in cash, and privately, upon leaving the room.

To recall, today's experiment consists of two parts. During **Part 1**, or the Earnings Stage, you will be asked to take a test. Based on your performance during Part1, you will be assigned one of two types: **Participant A or Participant B.** The type will remain unchanged throughout the experiment.

During **Part 2**, you will be randomly paired with another person of the other type and will be asked to accomplish a task that you will be given for each of 20 periods. Each period you will be given an initial endowment to accomplish the task. **Participants A will get an endowment of 36,000 Talers and Participants B will get an endowment of 36,000, 56,000, 72,000 or 90,000 Talers, respectively, dependent on their performance in Part 1.**

When you have finished Part 2 of the experiment, you will be asked to fill in a questionnaire. The answers you provide in this questionnaire are anonymous. Furthermore, your answers to this questionnaire will not affect your earnings during the experiment but I appreciate if you answer carefully. While you fill out the questionnaire I will prepare your earnings.

In the following pages, the experiment is described in detail.

Instructions Part 1

During this stage, you will be able to earn an appreciable amount of experimental money by answering questions from a test. The test will consist of 10 questions and all participants will have 10 minutes to answer them.

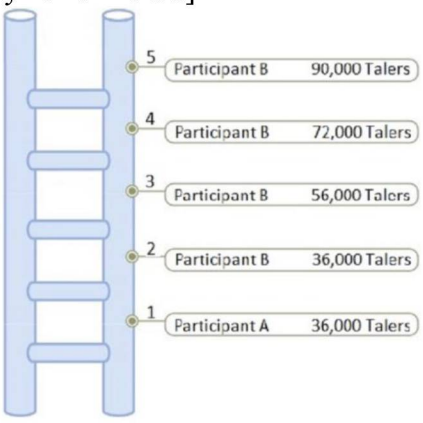
After you finish this test, all participants will be ranked based on the results of the test (the more correct answers you get, the higher your rank will be). If two or more participants answer the same number of questions correctly, the computer will randomly determine the rank order among them. Half of the participants – those with the lower ranks -- will be given the role of Participant A. The other participants will be given the role of Participant B, as shown in the table below. The role that you will be assigned will remain unchanged through the experiment.

Rank	Role	Initial endowment
'1' - '8'	Participant A	36,000
'9' – '10'	Participant B	36,000
'11' – '12'	Participant B	56,000
'13' – '14'	Participant B	72,000
'15' – '16'	Participant B	90,000

If you have finished reading press the "Continue" button on the computer screen.

Instructions Part 1.2.1

[were displayed on screens only for Officials]



You are presented with an image of a ladder with 5 rungs. Think of the ladder as representing where people stand in this experiment in terms of their income (initial endowment) and the prestige of the types assigned to them. On the bottom of the ladder there are participants of type A who get the initial endowment of 36,000 Talers and on the top – Participants of type B who get 90,000 Talers. Please place yourself on this ladder relative to the people at the very bottom and very top (please tick a box with the number of a rung where you stand in terms of your endowment and type).

Now imagine a society with the same socioeconomic hierarchy as described by the 5-rung ladder above. Please describe what your life in this society would be given your position on the social ladder by answering the following questions.

Please choose a number in the following scale from 0 to 5.

1. What goods would you buy?

- o 1 Only most necessary goods
- o 2
- o 3
- o 4
- o 5 Any, including luxury

2.1 What recreational activities would you have (how would you rest)?

- o 1 Only free ones, organized by myself
- o 2
- o 3
- o 4
- o 5 Any, including luxury

2.2 Please write an example(s) of such activity(s)

3 In what shops would you shop?

- 1 Cheapest ones, offering discounts
- 2
- 3
- 4
- 5 Exclusive luxury shops

4.1 What accommodation would you have?

- 1 The cheapest one
- 2
- 3
- 4
- 5 Luxury one(s)

4.2 Please write an example(s) of/shortly describe such accommodation(s)

5. What means of transport would you use every day?

- 1 Public transport
- 2
- 3
- 4
- 5 Luxury vehicle(s) with a personal driver

6. Now imagine yourself in a getting-acquainted interaction with one of the people from the TOP rung of the ladder. Think about how the DIFFERENCES OR SIMILARITIES BETWEEN YOU might impact what you would talk about, how the interaction is likely to go, and what you and the other person might say to each other. Would you have common interests/topics? Would you feel calm/confident/relaxed/anxious/shy/unsure/distant, etc?

Please write a few words about how you think this interaction would go and how would you feel.

Now imagine yourself in a getting similar interaction with one of the people from the LOWEST rung of the ladder.

Please write a few words about how you think this interaction would go and how would you feel.

Instructions Part 1.2.2 [were displayed on screens only for Firms]

You will be asked a few questions. Please provide your honest opinion. Your answers will stay anonymous

Please tick a number in the following scale from 0 to 5.

1 In your opinion, how important is studying economics at school/university for a representative citizen of the Czech Republic (given that economics is not her/his career choice)?

- o 1 Not at all
- o 2
- o 3
- o 4
- o 5 Very important

2 In your opinion, how important is studying mathematics at school/university for a representative citizen of the Czech Republic (given that mathematics is not her/his career choice)?

- o 1 Not at all
- o 2
- o 3
- o 4
- o 5 Very important

3 In your opinion, how important is studying psychology at school/university for a representative citizen of the Czech Republic (given that psychology is not her/his career choice)?

- o 1 Not at all
- o 2
- o 3
- o 4
- o 5 Very important

4 In your opinion, how important is studying languages at school/university for a representative citizen of the Czech Republic (given that languages is not her/his career choice)?

- o 1 Not at all
- o 2
- o 3
- o 4
- o 5 Very important

5 How many languages do you think a person in a modern society should speak fluently?

- o 0
- o 1
- o 2
- o 3
- o More than 3

6 Please write down three languages which you think are useful to know in a modern society. Start from the most important one and finish with the least important one.

7 Please write down which subject that you study/studied at school/university is/was most useful.

Instructions Part 2

This is a decision-making part. The payment you receive at the end of the experiment depends on the decisions you make. Moreover, you will be able to earn money for a charity. I will explain this in more detail below.

This part consists of 20 periods. At the beginning of the first period, you will be randomly matched to a player of the other type (A if you are B, B if you are A). The player you are matched with will remain unchanged throughout the experiment.

Once you have read the information in this Part carefully you will be asked to do several computerized control exercises, which are designed to check that you have understood the decision situation. Apart from that you will be able to practice during two trial periods before the payoff-relevant periods begin.

Before you proceed, make yourself familiar with Figure 1 **which is supplied on a separate sheet**. It is useful to **relate every explanation that follows below to Figure 1.** I also recommend you to highlight the line which corresponds to the role and endowment you got.

Decision Situation

Every period in this experiment consists of five stages, which will always take place in the following order:

Stage 1: Transfer or no transfer

Participant A decides whether he wants to transfer an amount *t, t*> 0, to Participant B. If he does, then the period is continued with Stage 2. If Participant A decides not to transfer a positive amount, i.e., t = 0, then the period continues with Stage 5.

Stage 2: The amount to be transferred

Participant A decides on the amount to be transferred to Participant B, *t*. The transferred amount *t* can be any whole number greater than zero and 36,000Talers ($0 \leq t \leq 36{,}000$).The period then continues with stage 3.

Stage 3: Acceptance or rejection of the transfer

Participant B then decides whether to accept the proposed transfer, *t*. If Participant B decides to accept it, the proposed amount is removed from Participant A's credit and added to Participant B's credit. The period then continues with Stage 4. If Participant B rejects the transfer, then the credits remain unchanged. The period is then continued with Stage 5.

Stage 4: Possibility of Getting Disqualified

If Participant B decided to accept the transfer in Stage 2, a number out of the range from 1 to 1000 is randomly drawn. If the number is 1, 2 or 3, then both Participant A and Participant B are disqualified (the probability of being disqualified is 0.003). That means that the experiment ends for these two players and all their previous earnings are canceled. (At the end of the experiment, both players receive only their show-up fee.) The two disqualified participants fill in a questionnaire until the experiment has ended. For the other participants, the experiment continues normally. If the randomly drawn number is 4, 5, ..., 999, or 1000 (which happens with probability 0.997), the period is continued with stage 5.

Stage 5: Participant B Chooses Between X and Y

Participant B chooses one of the alternatives *X* or *Y*. If Participant B selects alternative *X*, then his credit is increased by t and becomes (36,000/56,000/72,000/90,000 +*t*). The credit of Participant A is decreased by *t* and becomes (36,000 –*t*). The credit of the charity remains unchanged.

If Participant B selects alternative *Y*, then his credit is decreased by 6,000 Talers but is increased by *t* and becomes (30,000/50,000/66,000/84,000 +*t*). The credit of Participant A is increased by 20,000 and decreased by t and becomes (56,000 – *t*). The credit of the charity is decreased by 1,500 Talers.

There will be one charity that can benefit from this experiment. The charity starts off with a total of 200,000 Talers, which equals 900 CZK. The final donation depends on the decisions made by the participants in the experiment. The donation will be strictly anonymous; no mention will be made of either LEE or any participant of this experiment. The donation will be made online after the end of the experiment. The receipt for the donation will be sent to your email soon after the end of the experiment or can be found here:
https://www.dropbox.com/sh/0lx65mtcdukl7nx/AAAnWF22d-bO1879teoBoZX4a?dl=0
After Stage 5, the period has ended. The decision situation will be repeated for 20 periods.

- **Final earnings**

The earnings from **only one of 20 periods** will be converted into CZK and paid to you at the end of the experiment. The computer will randomly decide the period that will be paid out. As for the charity, the final donation will be equal (200,000Talers – n*1,500), where n is the number of times option Y is chosen **during 20 periods**.

- **Charities**

For this experiment, I have selected a total of five charities. You will be asked to select a charity at the beginning of Stage 2. At the end of the experiment, I will pick the charity selected by one randomly determined person.

A. UNICEF: Created by the United Nations. Its activities include promoting children's rights, and securing worldwide visibility for children threatened by poverty, disasters, armed conflict, abuse and exploitation.

B. Red Cross: Amongst its activities, it attempts to organize nursing and care for those who are wounded on the battlefield; it also supervises the treatment of prisoners of war.

C. Greenpeace: Focuses its campaigning on worldwide issues such as climate change, deforestation, overfishing, commercial whaling, genetic engineering, and anti-nuclear issues.

D. The Catholic charity Caritas Czech Republic: providing accommodation, health services, integration projects, and general help for the aged, disabled, migrants, prisoners, women with children, and the poor.

E. People in Need (Člověk v Tísni): is a humanitarian organization. It concentrates on human rights, alleviation of poverty and the reduction of national prejudices and xenophobia.

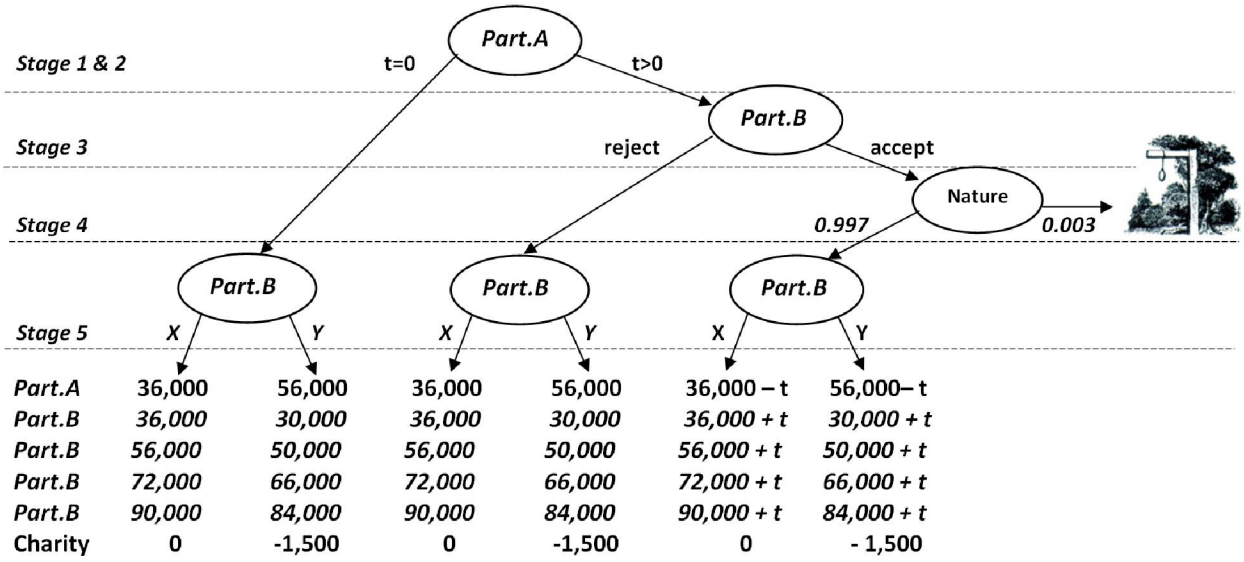


Figure 1: The experimental game tree

Notes: In the figure, Part.A – Participant A; Part.B – Participant B; the transfer is represented by t and X and Y are the two options that can be chosen by Participant B.

C Comprehension questions

[Comprehension questions were shown on the screen of the computers before the beginning of the Part 2 of the experiment. Once a participant typed the answer s/he saw the correct answer and explanation]

Question 1: Suppose Participant A has proposed a transfer of 8,000 Talers to Participant B. If Participant B accepts and the pair is not disqualified, what will be Participant B's earnings in Talers if option X is chosen?

Answer: Participant B's earnings in Talers will be: 56,000 + 8,000 = 64, 000

Question 2: What will be Participant A's earnings in Talers in this case?

Answer: Participant A's earnings in Talers will be: 36,000 - 8,000 = 28, 000

Question 3: What will be Participant B's earnings in Talers if option Y is chosen?

What will be Participant A's earnings in Talers if option Y is chosen?

Answer: Participant B's earnings in Talers will be: 50,000 + 8,000 = 58, 000

Participant A's earnings in Talers will be: 56,000 - 8,000 = 48, 000

Question 4: In this experiment, there are a total of 16 participants, such that there are 8 pairs. Suppose that in the first period there are 5 pairs in which Participant B chooses option Y and in the second period - 6 pairs. How many Talers will the charity lose in total after the second period ?

Answer: Charity will lose the cumulative amount for the two periods: 7,500 + 9,000 = 16,500 Talers

D Questionnaire (was shown on the screen of the computers at the end of the experiment

Thank you for participating in today's experiment. Please answer the following questions while the experimenter is preparing your earnings.

1 Please select your gender Male / Female

2 What is your Major?

3 What is your year of birth?

4 What is your current study level?

A. High school;

B. Bachelor or equivalent;

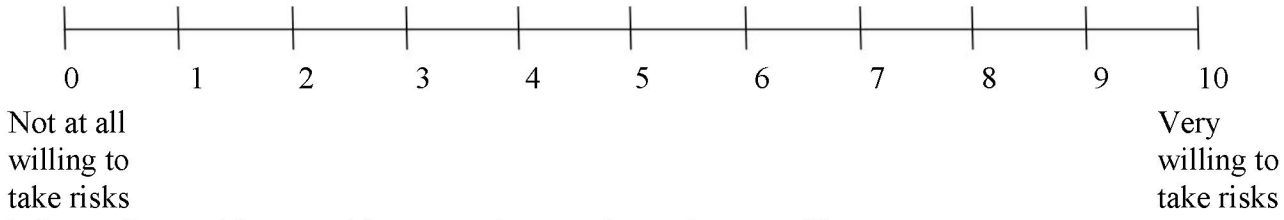
C. Master or equivalent;

D. PhD or equivalent;

5. How do you see yourself?

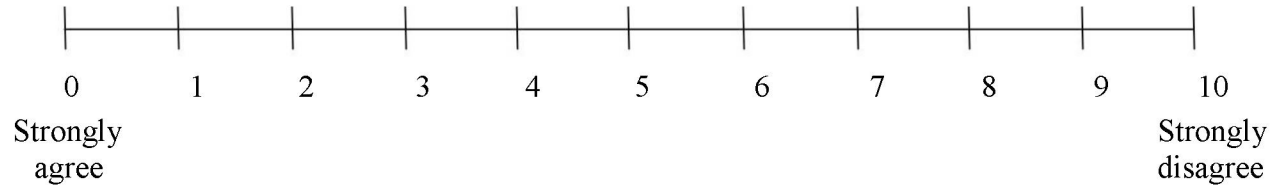
Are you generally a person who is fully prepared to take risk or try to avoid taking risk?

Please tick a number in the following scale, where the value 0 means not at all willing to take risks, and the value 10 means very willing to take risks‖.



6. Generally speaking, would you say that people can be trusted?

Please tick a number in the following scale, where the value 0 means: —strongly agree that people can be trusted, and the value 10 means: —strongly disagree that people can be trusted.



E Variable description and summary statistics

Table 1 Variable description and summary statistics (general for Treatment 1-5)

Variable Name	Variable Description	Descriptive Statistics									
		T1		T2		T3		T4		T5	
		Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
WT2	Dummy variable for treatment 2										
WT3	Dummy variable for treatment 3										
WT4	Dummy variable for treatment 4										
Male	Dummy variable. Equals one if gender is male	.737	.441	.688	.464	.467	.500	.462	.500	.536	.499
Age	Participant's age	23.895	5.063	23.563	4.982	23.467	2.709	22.308	1.591	23.38	3.45
Economics student	Dummy variable. Equals one if the participant is/was an economics student	.474	.500	.375	.485	.333	.472	.615	.487	.355	.479
Risk	A participant's self-assessment of to what extent she is prepared to take risk: 0 - not at all willing to take risks, and the value 10 - very willing to take risks‖	5.895	2.867	5.063	2.049	5.733	2.466	5.462	2.065	5.435	2.476
Highest degree	The highest academic degree achieved by the participant at the moment of the experiment): 1=High school; 2= Bachelor or equivalent; 3=Master or equivalent; 4=PhD or equivalent.	1.421	.675	1.438	.705	1.533	.619	1.462	.636	1.609	.675

Note: The summary statistics is provided for public officials only

Table 2 Variable description and summary statistics for SSES manipulation (for Treatment 5 only)

Variable Name	Variable Description	Descriptive Statistics	
		T5	
		Mean	St. Dev.
SSES	Categorical variable for self perceived socio-economic status, can take values 2 to 5	3.29	1.24
SSES_goods	Categorical variable for goods that one would buy when being of certain SSES, can take values 1 to 5	2.81	1.24
SSES_act	Categorical variable for recreational activities that one would have when being of certain SSES, can take values 1 to 5	2.59	1.39
SSES_shop	Categorical variable for shops in which one would shop when being of certain SSES, can take values 1 to 5	2.45	1.25
SSES_accomod	Categorical variable for accommodation one would have when being of certain SSES, can take values 1 to 5	2.61	1.13
SSES_transport	Categorical variable for means of transport one would use when being of certain SSES, can take values 1 to 5	1.94	1.37

Note: The summary statistics is provided for public officials only